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J. R. HIND, Esq., F.R.S., President, in the Chair.

Squire Thornton Stratford Lecky, Esq., 1, Morningside Road, Bootle, Liverpool;

James Henry Mitchiner, Esq., Burgos House, Sydenham Road North, Croydon; and

The Rev. T. R. Terry, M.A., Magdalen College, Oxford;

were balloted for and duly elected Fellows of the Society.

On the Determination of the Value of the Parallactic Inequality in the Motion of the Moon. By James Campbell, Esq., and Edmund Neison, Esq.

In the *Monthly Notices* for November 1880, there is a paper by Mr. Stone, F.R.S., criticising a paper of ours which appeared in the *Monthly Notices* for May and June 1880, and is entitled, "On the Determination of the Solar Parallax by means of the Parallactic Inequality in the Motion of the Moon." This paper by Mr. Stone calls for a reply from us.

Mr. Stone's paper may be divided into three portions, under the following heads—

(1) An account of the method adopted by him in his determination of the value of the parallactic inequality published in 1867.

(2) A demurrer to the manner in which the effect of the variation in the irradiation at the lunar limb is deduced in our investigation.

(3) A criticism of the hypothesis of the possible existence of an equality of long period in the apparent value of the coefficient of the parallactic inequality.

These three portions we propose to take in order.

(1) Mr. Stone states that we have formed an incorrect idea of the method employed by him in the investigation of which the only published account is the short abstract in the *Monthly Notices* for May 1867, an account which, though professing to describe the method which was followed, yet makes no reference to the additional precautions and corrections which it seems were actually employed by Mr. Stone. Through interpreting in the most natural manner the account given in this abstract, it seems that its imperfect nature has led us to form an incorrect idea of the method employed. This we must regret. Yet we submit that the carefully guarded manner in which we describe, on pp. 407–409 of our paper, the conclusions which we draw as to the probable method followed by Mr. Stone, and the clear manner in which we restrict our criticisms to Mr. Stone's value as thus hypothetically interpreted, must be regarded as effectually vindicating us from the unnecessary strictures in this paper by Mr. Stone.

We accept without demur Mr. Stone's statement that the specific corrections hypothetically applied by us to his result are really inapplicable. This naturally follows from the fact that Mr. Stone's investigation had a different basis to what we had been led to imagine. We learn now from Mr. Stone what is the real basis of his investigation, and consequently can form, now, a clear idea of the value of the result. The only conclusion which it seems can be drawn is that the value obtained by Mr. Stone must require the application of a considerable number of large corrections, and even then can only be regarded as of most imperfect nature.

Thus, from Mr. Stone's account, it seems, that actually passing over the great mass of indirect comparisons between observation and Hansen's tables, he employed the far inferior series of comparisons between observation and Burckhardt's tables, merely applying some small corrections to render these latter tables somewhat less faulty. Further, it seems that in round numbers more than three-fourths of the observations employed by Mr. Stone were compared with Burckhardt's tables, and only one-fourth with Hansen's tables. Moreover, it would appear that some two-thirds of the observations made use of by Mr. Stone were made with the Greenwich altazimuth instrument, and only about one-third with the transit circle.

Now, the errors in Burckhardt's tables are so large and so numerous that, to render the tables fit for use in deducing corrections to any of the lunar inequalities, it is absolutely essential that a considerable number of corrections should be applied. Moreover, in most cases these corrections must be applied to the individual observations, and cannot be adequately applied to the mean results. This is a point which we strongly urge, and is founded on the considerable experience which one of us has had in the comparison of these tables with observations. The errors

introduced by the employment of these tables are rectified in a very small degree by merely applying a correction to the mean results for "errors in the coefficient of Burckhardt's variation, annual equation, and term 3D." (Mr. Stone does not tell us what were the separate corrections he deemed these coefficients to require.)

Besides these errors in the values of the coefficients of the terms actually employed by Burckhardt in his Lunar Tables, these tables are vitiated by the imperfections due to the omission of numerous terms found by subsequent theorists to have sensible coefficients. These omissions will introduce systematic errors into any results based on his tables. It may be urged that in the mean of nine or thirteen years' observation these errors may mutually destroy each other. They *may*: but this is a point which must be proved, not assumed.

But the greatest objection to the results obtained by Mr. Stone lies in the fact that they practically are founded on the altazimuth observations, and mainly on the altazimuth observations compared with Burckhardt's tables.

The altazimuth observations are liable to so many systematic errors that they are entirely unfitted for use in determining the value of the parallactic inequality, especially when they are compared with faulty tables. The observations for the positive and negative values of the parallactic inequality are systematically made with this instrument under entirely different conditions, and are affected, therefore, by every systematic error inherent to such a condition. The observations at the two quarters are not only made at different times of the night, one long before and the other usually long after midnight, but they are made on opposite sides of the meridian, and in entirely different portions of the heavens. All these must tend to introduce grave systematic errors. Further, from the conditions under which the observations are made, not only will there be grave systematic errors introduced through the variations in the semi-diameter of the Moon, but there will be also serious systematic errors introduced through the corrections both for parallax and refraction. The difficulties in dealing with the variations in the apparent diameter of the Moon seem enormous. The diameter of the Moon derived from observations in altitude is known to be entirely discordant with that derived from observations in azimuth, differing, in fact, by over one second; and this, whilst conclusively establishing the existence of serious systematic errors in the observations, renders it quite inadmissible to apply a constant correction for semi-diameter, as is done by Mr. Stone. Which is the true correction to be employed, that furnished by the observations in altitude, or that furnished by the observations in azimuth? The resulting values of the parallactic inequality will differ by over half a second. And if neither, but some intermediate value, how can it be determined what intermediate value is correct? Moreover, though the personalities in observing diameters with the transit circle have

been shown to be small, those in observations with the altazimuth are known to be very large.

For this reason we consider that the altazimuth observations are unfitted for use for the purpose of determining the value of the parallactic inequality in the motion of the Moon, unless care be taken to eliminate all these sources of systematic error by the application of adequate corrections to the separate observations; and we believe that in this opinion we shall be supported by all those astronomers who have had experience in the comparison of these altazimuth observations with an adequate lunar theory.

As, from Mr. Stone's account of his investigation, he only applied to mean results assumed corrections for errors in the variation, annual equation, and term 3θ (Mr. Stone's 3D), together with a constant correction for semi-diameter, it would appear that the corrections he has applied are altogether inadequate to properly eliminate the various sources of systematic error affecting the altazimuth observations.

(2) The second point raised by Mr. Stone is a very simple one. In our paper we determined the coefficient of the irradiation constant from the equations—

$$\delta P + 0{\cdot}75\ \delta I = \text{apparent value of parallactic inequality from summer observations.}$$

$$\delta P + 0{\cdot}25\ \delta I = \text{apparent value of parallactic inequality from winter observations.}$$

Mr. Stone objects that the values taken for the coefficients of δI are assumptions, and that their correctness must be proved. He shows that by varying the ratio between these two coefficients you make important changes in the value of the true parallactic inequality. This objection would be perfectly valid were the ratio between these two coefficients a mere assumption, as imagined by Mr. Stone. But this is not so. On p. 445 of our paper it is clearly stated that these coefficients, and therefore the ratio between them, were derived from the observations themselves. This ratio of three to one arises practically from the fact that three times as many observations are made on a bright sky in the summer as are made in the winter. The actual figures given by the observations range usually within a small distance on either side of the mean values 0·25 and 0·75.

It has been already stated that the coefficient i of the constant δI was assumed to vary directly as the brightness of the sky at the epoch of the transit of the Moon. This leads to results the same as employed by Prof. Newcomb. But even if a different law be assumed, it will be found that the ratio between the two coefficients is not materially affected. Originally, a somewhat different law was assumed by one of us in these investigations, and this result was found to hold; but it was abandoned because, on comparison with the results furnished by the observa-

tions, it was found that the variation in the irradiation increased at a greater rate than had been assumed.

There is, however, some additional information on this point which may now be given; which, indeed, would have been included in our former paper had it not been deemed inadvisable to increase its already great length. This is a proof of the accuracy of the law we have assumed for the variation in the irradiation at the limb; the law being that the coefficient i varies directly as the brightness of the sky, or that the irradiation at the limb varies directly as the contrast in brightness between the limb of the Moon and the sky it is seen against, or inversely as the brightness of the sky.

In the reduction of the Greenwich lunar observations which has been undertaken by one of us, every observation is supposed to require the small correction $i\delta$I, where i varies from 0 to 1·5 according to the law stated. Let the observations be divided into four groups; those for which the value of i lies between the limits—

$i = 1{\cdot}5$ and	$i = 1{\cdot}2$	mean $i = 1{\cdot}35$
$i = 1{\cdot}2$	$i = 0{\cdot}9$	$i = 1{\cdot}05$
$i = 0{\cdot}9$	$i = 0{\cdot}3$	$i = 0{\cdot}60$
$i = 0{\cdot}3$	$i = 0{\cdot}0$	$i = 0{\cdot}15$

From each of these groups let δI be determined, and if each group yields the same value, then the assumed law must accurately represent the true law; but if they be discordant, then the coefficient i must be made to vary according to some different law which will make all these values alike. It is not difficult to eliminate from the observations for any group of years nearly all the other corrections, leaving merely δI. The Greenwich observations for the nine years 1868–1876 were thus reduced, leaving out the last group, as the coefficient of i is too small to give trustworthy results. The number of observations included in the first group were but small, so that its weight was small. Of course, the real importance lies in the results obtained from the two middle groups, for if these give identical values of δI, then the assumed law is proved. The results are—

For mean value of $i = 1{\cdot}35$	$\delta I = -1''{\cdot}30$	Weight 1
$= 1{\cdot}05$	$= -1{\cdot}25$	5
$= 0{\cdot}65$	$= -1{\cdot}32$	7

This completely confirms the sufficiency of the assumed law. It may be added that these results are based on the complete series of meridian results for the period specified, or on nearly one thousand observations.

Mr. Stone has also criticised the value we find for δI. He states that even after applying the constant corrections given by us, the separate values are very discordant. Apparently

Mr. Stone has overlooked the following paragraph in our paper (pp. 464, 465)—

"When the values assigned to the corrections δV, δM, δK, δ*l*, come to be substituted in the expressions for δI during the different years, it will be found that there still remain considerable outstanding errors. These are partly due to the fact that constant values have been assigned to these corrections, whereas the observations show that they are subject to some variation, due to the fact that Hansen's tables require one or two more corrections not included in the preceding expression. Strictly speaking, in each year should have been used the values found for δV, δM, δK, δ*l*, from the observations of that year, as in that manner these neglected corrections would have been practically eliminated. The method actually adopted for determining the value of P gives results which are sensibly as accurate as if the stricter method had been employed, for the corrections are multiplied by such small coefficients that slight variations may be neglected. When the expressions for δI are considered, the use of the stricter method markedly reduces the values of the outstanding errors in the years 1876, 1869, 1868." [Almost the only large outstanding errors.] "But it must be remembered that the apparent errors of these years are much exaggerated by their being derived from equations in which δI is multiplied by a small coefficient, so that in dividing by this coefficient the apparent errors are increased to double, triple, or even quadruple their true amount. In the value for the irradiation which has been derived from the observations and actually employed, all the equations are grouped together, and appear with their proper weight according to the largeness of their coefficients of δI. The result, therefore, is entitled to far more weight than may appear from a consideration of the outstanding discordances, so many of which have been thus exaggerated."

It may be added that the results were computed on both systems, and found to agree. Since then the principal cause of the variation in the apparent value of δM and δK (for the latter, Prof. Newcomb's value was used in our paper) has been found to arise from the need of a correction to the term with the argument α or the mean anomaly of the Moon. This was surmised in our paper.

Therefore, as it has been shown that the law of the variation in the irradiation at the limb, the actual amount of this variation, and the ratio between the coefficients with which it appears in the final equations are all derived directly from the observations themselves, it must be held that the correction for the effect of this variation in irradiation is also furnished directly by the observations, and is altogether free from that arbitrary character imagined by Mr. Stone.

We hold, therefore, that we have vindicated this portion of our investigation from the demurrer raised against it by Mr. Stone.

(3) In his criticisms on the portion of our paper dealing with the hypothetical forty-five-year inequality in the apparent value of the parallactic inequality, Mr. Stone seems to have entirely misunderstood our position, and it might be imagined that we supposed our paper to contain a proof of the existence of this new term. But that is a complete misconception, as throughout we refer to this inequality as purely hypothetical, and one whose existence has yet to be proved. (See for instance page 467 lines 16–28, and page 468 lines 6–8.) As we so clearly state in our paper, we do not regard the existence of this term as established, for reasons we have already given, but we do consider that there is strong evidence in favour of the possible existence of such an inequality.

Let us recapitulate this evidence.

Sir G. B. Airy had reduced on a uniform system, and by reference to uniform lunar tables, the entire series of Greenwich lunar observations made between 1750 and 1851; and from these observations there have been deduced twenty-one values of the coefficient of the apparent parallactic inequality, these values being strictly comparable *inter se*, and each being based on some six or seven hundred observations, spread over a period of nine years. When these values come to be compared, it is found that they exhibit grave discordances amounting to over a second of arc in many cases, but that these discordances are obviously periodical in form. If these discordances be assumed to be due to the existence of a hitherto unsuspected term in the motion of the Moon, giving rise to an apparent inequality of forty-five years' period in the values found for the parallactic inequality, all these discordances disappear from twenty of these nine-year groups, and the only discordance is for the small group of years 1847–1851. There can be no doubt about this constituting strong evidence in favour of the existence of such an inequality. It is not the case, as imagined by Mr. Stone, that the only evidence in its favour lies in the early observations made prior to 1816; for a reference to the table on page 403 of our paper shows serious discordances in the observations between 1816 and 1846 which are completely removed when the inequality is introduced. These as strongly support its existence as any of the earlier groups. We have, therefore, the observation for the ninety-seven years 1750–1846 in its favour, and only those of the five years 1847–1851 against it. Mr. Stone urges that the observations of the period 1847–1851 are superior to those of the preceding century. Undoubtedly, but they are not sensibly superior to those made between 1835–1846, or much better than those between 1816–1835.

Further, it is found that when the observations made between 1862–1876 are reduced and compared with Hansen's tables as corrected, they give a value of the apparent parallactic inequality which is in perfect accord with the assumed existence of this forty-five-year inequality. It is here, however, that the weak

point appears, and is duly noted by us; these observations are not only made with a different instrument, but they are not referred to the same tables as the observations employed by Sir G. B. Airy, and the two results are not *strictly comparable*. It is true that the fundamental differences between the two tables (Airy and Hansen *both corrected from observation*) are not so large as to render them likely to be of much importance, and the two instruments were of analogous nature, but these differences do decrease the weight to be attached to the close accord found to exist between the results.

Mr. Stone, however, to show that this hypothetical inequality has no real existence, brings forward the values which he has deduced for the apparent value of the parallactic inequality, by comparing the altazimuth observations of the Moon for 1848–1861 with Burckhardt's lunar tables, and for the years 1862–1878 with Hansen's tables. But as these observations are not only made with an entirely different description of instrument, they are neither comparable with the results obtained by Sir G. Airy or by ourselves from the meridian observations, nor, as they are referred to entirely different tables, are they even comparable with each other. For, be it remembered, these values which are being discussed are not the values of the coefficient of the true parallactic inequality, but of the apparent inequality which is affected by the variations in the irradiation at the limb, and may be therefore entirely different in different instruments. If the results quoted by Mr. Stone were comparable, then they would be utterly discordant with the hypothesis of a forty-five-year inequality. But they most certainly are not comparable. The results deduced through the exceedingly faulty tables of Burckhardt, and affected by all the consequent systematic errors, cannot be compared with the results deduced through the more exact tables of Hansen without the application of a far more extensive series of corrections than has been applied by Mr. Stone;* and the results deduced from the altazimuth observa-

* With the view of showing the justness of this statement that the corrections applied by Mr. Stone to the results from Burckhardt's tables are entirely insufficient to eliminate the errors introduced by the imperfection in the theoretical basis of the tables, we have computed the true corrections to be applied to the results for the three years 1854–56, taking into account the errors in some twenty-two terms, instead of only three. The results are:—

	Stone. Correction for error in 3 terms.	Neison and Campbell. Correction for remaining 18 terms.	
1854	$= +1''\cdot56$	$+1''\cdot47$	$= -1''\cdot85$ Sum $= -0''\cdot38$
1855	$= +1\cdot56$	$+1\cdot62$	$= -1\cdot96$ $= -0\cdot34$
1856	$= +1\cdot56$	$+\ \cdot98$	$= +0\cdot97$ $= +1\cdot95$

In these particular years the errors are not near these maximum values, and it may be stated that the constant correction used by Mr. Stone of $+1''\cdot56$ is a variable quantity which may take all values from about $+3''\cdot5$ and $-1''\cdot5$.

tion cannot be compared with the results of the meridian observations, as they are affected by constant errors due to the variation in the irradiation at the limb, which must differ in the two different classes of instruments, even had not the results derived from the altazimuth observations been affected by serious systematic errors.

Mr. Stone, however, has appealed to the observations of the period 1850–1860 as being decisive against this forty-five-year inequality. Are they really so? This is a matter admitting of proof, for there exists a considerable number of observations made with the transit circle and indirectly compared with Hansen's tables. They form, in fact, the very data which we had imagined Mr. Stone to have employed in his investigation, being so much superior to those actually employed by him. These observations are given in the volume of Greenwich Observations for 1859, and from them we select all the observations made with the transit circle, and comprised within the years 1851–1858. They furnish us with results strictly comparable with those for the years 1862–1876 made use of by us in our previous paper.

The results are as follows:

1851	43 observations	$\delta(P) = -2''\cdot03$
1852	31	$= -3\cdot17$
1853	42	$= -1\cdot50$
1854	45	$= -2\cdot45$
1855	36	$= -1\cdot80$
1856	37	$= -2\cdot49$
1857	38	$= -2\cdot46$
1858	53	$= -2\,65$
	325 observations	$\delta(P) = -2\cdot32$

To reduce this result to what would have been obtained had they been compared directly with Hansen's tables, it is necessary to apply the constant correction for the difference between the two tables at midnight and at the epoch of observation. This amounts to (see page 401 of our paper)—

$$\tfrac{1}{9}\,\Delta V + \tfrac{1}{25}\Delta Q = -0''\cdot52.$$

The result is then the value which would have been obtained had the observations been compared directly with Hansen's tables. It is

$$(P) = -\{126''\cdot46 - 2''\cdot32 - 0''\cdot52\} = -123''\cdot62.$$

This value has now to be corrected for the errors in Hansen's

tables. These have been approximately calculated, and the mean correction for the eight years is

$$+0''\cdot10\,\delta V - 0''\cdot13\,\delta M + 0''\cdot07\,\delta A = +0''\cdot14.$$

The correction for the term of the form $\delta K \sin k$, which gives the correction required by Hansen's tables for errors in the equation of the centre and the Jovian evection, was calculated, but in the mean of the eight years nearly vanishes; it is—

$$+\kappa\,\delta K \quad = -0''\cdot03.$$

For the correction required by the semi-diameter we have taken that given in the Greenwich Observations for 1863, namely

$$+s\,\delta S \quad = -0''\cdot18.$$

This is somewhat greater than the value found by one of us for the period 1851–1858. The sum of these corrections amounts to

$$+0''\cdot14 - 0''\cdot03 - 0''\cdot18 \quad = -0''\cdot07.$$

Therefore, the value of the apparent parallactic inequality will be

$$(P) = -\{123''\cdot62 - 0''\cdot07\} = -123''\cdot55.$$

This is the result furnished by the 325 meridian observations made during the eight years 1851–1858.

It is easy now to compare this result with those obtained in our previous paper. We have added the correction required for the forty-five-year inequality—

1851–1858	$(P) = -123''\cdot55 - \frac{4}{7}B$
1862–1869	$= -125''\cdot09 + \frac{4}{5}B$
1870–1876	$= -125''\cdot13 + \frac{4}{5}B.$

Giving to B its value, $B = +1''\cdot20$, these become

1851–1858	$(P) = -124''\cdot23$
1862–1869	$= -124''\cdot09$
1870–1876	$= -124''\cdot13.$

To these may be added Airy's value

1751–1846	$(P) = -124''\cdot13.$

It will be evident, therefore, that the entire series of meridian

observations unite in supporting this hypothesis of a forty-five-year inequality. More than that we do not wish to say at present, unless it is to add that if this inequality really exists it must be possible to find out its origin and calculate from theory its value.

In reducing these observations, care was taken to determine the value they furnish for the irradiation constant δI so as to enable the value of the true coefficient of this parallactic inequality to be obtained. They gave the value

$$0{\cdot}50\,\delta I = -1''{\cdot}73 + \kappa\delta K + t\delta T.$$

The value of the correction depending on the term $\delta K \sin k$ was calculated, and the mean for the eight years was

$$\kappa\delta K = +0''{\cdot}46.$$

The term depending on the factor $t\,\delta T$ or on the systematic errors in the Greenwich clock star places, was calculated to be approximately

$$t\delta T = +0''{\cdot}72.$$

Hence

$$0{\cdot}50\,\delta I = -1''{\cdot}73 + 1''{\cdot}18 = -0''{\cdot}55.$$

Therefore

1851–1858	$\delta I = -1''{\cdot}10$
1862–1869	$= -1{\cdot}06$
1870–1876	$= -1{\cdot}18.$

The weight of the first of these is considerably smaller than that of the last two groups.

Employing these values of the irradiation correction, we have

1851–1858	$\delta P = (P) + 0{\cdot}44\,\delta I = -124''{\cdot}72$
1862–1869	$= (P) + 0{\cdot}46 \quad = -124{\cdot}62$
1870–1876	$= (P) + 0{\cdot}50 \quad = -124{\cdot}66.$

The accordance between the different results is complete, and leaves it necessary for us to add nothing.

(4) In concluding this paper it may be as well perhaps to state that further information on the true value of the parallactic inequality and on the rate and amount of the variation in the irradiation at the limb, but based on the entire available Greenwich Observations, will be furnished in all detail in a memoir on the corrections required by Hansen's lunar tables which is now

approaching completion in the hands of one of us. This memoir will contain an investigation of the systematic errors of the altazimuth observations.

Arkley Observatory,
Barnet, Herts:
1881, *March* 10.

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*Effect on the Moon's Movement in Latitude, produced by the slow Change of Position of the Plane of the Ecliptic.** By Sir G. B. Airy, K.C.B., Astronomer Royal.

It appears that, in consequence of the action of the Planets, the position of the plane of the orbit round the Sun, described by the centre of gravity of the luno-terrestrial system, undergoes a continual change; which, as estimated for a great number of years, may without sensible error be considered as a uniformly progressive change. The plane of the orbit in the year 1900+T years crosses the plane of the orbit in the year 1900 in a line whose longitude (as estimated from the zero of longitude customary in astronomy) is 172° 50′ nearly; and the plane of the orbit is inclined annually by 0″·479, in such a direction that the portion of the plane which is in longitude 262° 50′ is carried towards the north pole of the ecliptic of the year 1900. (Le Verrier, *Annales*, tome ii. p. 105.)

There is no reason to suppose that this small change in the position of the Sun's apparent orbit will produce any discoverable effects on the ecliptic-longitude or radius vector of the Moon; but we may properly examine what is its effect on the Moon's latitude, as referred either to the ecliptic-plane of 1900 or to that of 1900+T years.†

Now, referring all forces to the plane of the ecliptic in the year 1900, and putting z_s for the linear elevation of the Sun at any time above that plane; v being the geocentric longitude of the Moon, and V the longitude of the Sun as viewed from the luno-terrestrial centre of gravity, and other letters having their usual acceptation: it is easily found that—

* On the general question here considered, see *Mécanique Céleste*, Book vii., Chap. i. Also Cayley, "A third Memoir on the Problem of Disturbed Elliptic Motion" (*Mem. R.A.S.* vol. xxxi. [1863], pp. 43–56), and Hansen's *Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen* (Leipzig, 1862). Laplace's conclusion is that the Motion in regard to the variable ecliptic is the same as it would have been in regard to a fixed ecliptic. Hansen's correction of this result is cited in the footnote further on, p. 271.—Ed.

† It appears (p. 491 of the *Darlegung*) that the change produces also a long inequality in the Moon's longitude, the ratio of the coefficients being nearly 8 sin $\frac{1}{2}$J : 1, or numerically about 0·36; but the amount of the irregularity in latitude is not sensibly affected by this circumstance.—Ed.

Force in z depending on the Sun's elevation above the ecliptic of 1900

$$= +3\frac{\sigma}{A^3}\cdot\left(\frac{A}{R}\right)^3\cdot\frac{r}{a}\cdot\cos 1\cdot\cos\lceil v-V\rceil\cdot\frac{z_\sigma}{R}.$$

The value of $\frac{\sigma}{A^3}$, using the same constants which I have used in a preceding paper, is ·0055953, and $+3\frac{\sigma}{A^3} = +\cdot 016786$.

For the other factors which contain inequalities of the movements of the Earth and Moon, I propose to neglect all those periodic terms which are not strictly and necessarily connected with the present problem, or those whose periodic times are not simply related to those of the principal terms. The only inequalities which are so connected are those depending on $\lceil v-V\rceil$ or on $\lceil nt-Nt\rceil$, where

$$\frac{N}{n} = \frac{\text{Sun's mean angular motion}}{\text{Moon's mean angular motion}} = \cdot 0748013.$$

For the terms which contain other inequalities, I use only mean values of the coefficients.

Thus we have

$$\left(\frac{A}{R}\right)^3 = 1\cdot 0004215.$$

$$\frac{r}{a}\cdot\cos 1 = \cdot 9995477 - \cdot 0075470\cdot\cos\lceil 2nt-2Nt\rceil.$$

$$\left(\frac{A}{R}\right)^3\frac{r}{a}\cdot\cos 1 = \cdot 9999692 - \cdot 0075501\cdot\cos\lceil 2nt-2Nt\rceil.$$

For the two remaining terms, I shall express their value not singly, but in combination. Now, $\frac{z_\sigma}{R}$ is the value (referred to radius) of the Sun's apparent angular elevation above the ecliptic of 1900, or is

$$= +\cdot 479\times\sin 1''\times\text{number of years after 1900}\times\sin\lceil V-C\rceil,$$

$$= +\cdot 479\times\sin 1''\times\frac{t}{84}\times\sin\lceil V-C\rceil, \text{ (84 being the number of units of time in a Julian year),}$$

where $C = 172^\circ\ 50'$.

I shall use the letter B for $+\cdot 479\times\sin 1''\times\frac{1}{84}$. Then the Sun's angular elevation above the ecliptic $= Bt.\sin\lceil V-C\rceil$; and the product of the two last terms in the expression for "Force in z" is

$$\frac{Bt}{2}\times\{+\sin\lceil v-C\rceil-\sin\lceil v-2V+C\rceil\}.$$

Now, $v = nt+\cdot 01148\ .\ \sin\lceil 2nt-2Nt\rceil$ (including only those

small terms of which we have spoken), and V has no term depending on $\lceil nt-\mathrm{N}t \rceil$; and the product of the two terms therefore is

$$+\frac{\mathrm{B}t}{2}\times\left\{\begin{array}{l}+\sin\lceil nt-\mathrm{C}\rceil-\sin\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil\\ +(\cos\lceil nt-\mathrm{C}\rceil-\cos\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil)\times\cdot01148\,.\,\sin\lceil 2nt-2\mathrm{N}t\rceil\end{array}\right\}$$

$$=\frac{\mathrm{B}t}{2}\times\left\{\begin{array}{l}+\sin\lceil nt-\mathrm{C}\rceil-\sin\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil\\ +\cdot00574\,.\,\sin\lceil 3nt-2\mathrm{N}t-\mathrm{C}\rceil+\cdot00574\,.\,\sin\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil\\ -\cdot00574\,.\,\sin\lceil 3nt-4\mathrm{N}t+\mathrm{C}\rceil-\cdot00574\,.\,\sin\lceil nt-\mathrm{C}\rceil\end{array}\right\}$$

Multiplying this by

$$+\cdot016786\times\{+\cdot99997-\cdot00755\,.\,\cos\lceil 2nt-2\mathrm{N}t\rceil\}$$

(found above for $3\frac{\sigma}{\mathrm{A}^3}\times\left(\frac{\mathrm{A}}{\mathrm{R}}\right)^3\cdot\frac{r}{a}\cos\mathrm{l}$), we have for the force in z,

$$+\cdot0083930\,.\,\mathrm{B}t\times\left\{\begin{array}{l}+\cdot99997\,.\,\sin\lceil nt-\mathrm{C}\rceil-\cdot99997\,.\,\sin\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil\\ +\cdot00197\,.\,\sin\lceil 3nt-2\mathrm{N}t-\mathrm{C}\rceil+\cdot00951\,.\,\sin\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil\\ -\cdot00197\,.\,\sin\lceil 3nt-4\mathrm{N}t+\mathrm{C}\rceil-\cdot00951\,.\,\sin\lceil nt-\mathrm{C}\rceil\end{array}\right\}$$

$$=\mathrm{B}t\times\left\{\begin{array}{l}+\cdot0083130\,.\,\sin\lceil nt-\mathrm{C}\rceil-\cdot0083130\,.\,\sin\lceil nt-2\mathrm{N}t+\mathrm{C}\rceil\\ +\cdot0000166\,.\,\sin\lceil 3nt-2\mathrm{N}t-\mathrm{C}\rceil-\cdot0000166\,.\,\sin\lceil 3nt-4\mathrm{N}t+\mathrm{C}\rceil\end{array}\right\}$$

This is the expression for the force which is to be used on the first side of the Equation of the Factorial Table.*

The equation is the following—

$$\text{Force}=\left\{\begin{array}{l}+\ \delta\mathrm{l}\times(+1\cdot007945+\cdot0428\,.\,\cos\lceil 2nt-2\mathrm{N}t\rceil)\\ +\frac{d\delta\mathrm{l}}{dt}\times(\ \ \cdot000000+\cdot0278\,.\,\sin\lceil 2nt-2\mathrm{N}t\rceil)\\ +\frac{d^2\delta\mathrm{l}}{dt^2}\times(+\ \cdot999545-\cdot0075\,.\,\cos\lceil 2nt-2\mathrm{N}t\rceil)\end{array}\right\}.$$

A very little trial and consideration will show that the form to be assumed for $\delta\mathrm{l}$, whose substitution in the last expression will produce such a "force" as was found above, must be the following—

* The Factorial Table to which reference is made here, is a Table embodying three equations; which are prepared by forming the three equations of motion, each of which is expressed in terms of r, v, and z; and differentiating these equations (including both the geometrical ordinates, and the terrestrial and solar mechanical forces due to these geometrical ordinates) in such a manner as to exhibit the magnitudes of new forces which must be introduced in order to produce ordinates $r+\delta r$, $v+\delta v$, $z+\delta z$. I hope shortly to publish in detail, in a "Numerical Lunar Theory," the steps of formation of these equations. In the present instance, only one of the equations of motion is employed, and it is brought to its simplest state by neglecting all variable quantities not necessarily involved in the problem.

$$\delta l = \left\{ \begin{array}{l} +Pt \,.\, \sin \left| \overline{nt - C} \right| + Qt \,.\, \sin \left| \overline{nt - 2Nt + C} \right| + Rt \,.\, \sin \left| \overline{3nt - 2Nt - C} \right| + St \,.\, \sin \left| \overline{3nt - 4Nt + C} \right| \\ +\ p \,.\, \cos \left| \overline{nt - C} \right| +\ q \,.\, \cos \left| \overline{nt - 2Nt + C} \right| +\ r \,.\, \cos \left| \overline{3nt - 2Nt - C} \right| +\ s \,.\, \cos \left| \overline{3nt - 4Nt + C} \right| \end{array} \right\},$$

where, in each line, the third and fourth terms are much inferior in magnitude to the first and second, and their products with the small terms of the Factorial Equations are to be neglected. By differentiating δl we obtain

$$\frac{d\delta l}{dt} = \left\{ \begin{array}{rr} +Pt \,.\, \cos \left| \overline{nt - C} \right| & +\cdot 8504 \,.\, Qt \,.\, \cos \left| \overline{nt - 2Nt + C} \right| \\ +(P - p) \,.\, \sin \left| \overline{nt - C} \right| & +(Q - \cdot 8504 \,.\, q) \,.\, \sin \left| \overline{nt - 2Nt + C} \right| \\ +2\cdot 8504 \,.\, Rt \,.\, \cos \left| \overline{3nt - 2Nt - C} \right| & +2\cdot 7008 \,.\, St \,.\, \cos \left| \overline{3nt - 4Nt + C} \right| \\ +(R - 2\cdot 8504 \,.\, r) \,.\, \sin \left| \overline{3nt - 2Nt - C} \right| & +(S - 2\cdot 7008 \,.\, s) \,.\, \sin \left| \overline{3nt - 4Nt + C} \right| \end{array} \right\},$$

$$\frac{d^2\delta l}{dt^2} = \left\{ \begin{array}{rr} -Pt \,.\, \sin \left| \overline{nt - C} \right| & -\cdot 7231 \,.\, Qt \,.\, \sin \left| \overline{nt - 2Nt + C} \right| \\ +(2P - p) \,.\, \cos \left| \overline{nt - C} \right| & +(1\cdot 7008 \,.\, Q - \cdot 7231 \,.\, q) \cos \left| \overline{nt - 2Nt + C} \right| \\ -8\cdot 1247 \,.\, Rt \,.\, \sin \left| \overline{3nt - 2Nt - C} \right| & \\ +(5\cdot 7008 \,.\, R - 8\cdot 1247 \,.\, r) \,.\, \cos \left| \overline{3nt - 2Nt - C} \right| & +(5\cdot 4016 \,.\, S - 7\cdot 2944 \,.\, s) \,.\, \cos \left| \overline{3nt - 4Nt + C} \right| \end{array} \right\}.$$

It is readily seen that wherever p appears it is accompanied with a multiple of P: and so for q, r, s. Then, when P, Q, R, S, are found from the terms which multiply t, and are substituted in those which do not contain t, but which are easily solved, p, q, r, s, will be directly deduced from them. We may therefore, in the first instance, omit entirely the terms independent of t. We proceed to substitute the terms depending on t in the factorial equation.

External force, representing the physical assumptions; or First Side of the Factorial Equation :—

$$= Bt \times \{+\cdot0083130 \cdot \sin\overline{|nt-C|} - \cdot0083130 \cdot \sin\overline{|nt-2Nt+C|} + \cdot0000166 \cdot \sin\overline{|3nt-2Nt-C|} - \cdot0000166 \cdot \sin\overline{|3nt-4Nt+C|}\}.$$

Corresponding terms of the Second Side of the Factorial Equation, including only multiples of t :—

$$= \left\{\begin{array}{l} +\{+1\cdot007945+\cdot0428 \cdot \cos\overline{|2nt-2Nt|}\} \\ \qquad \times t \times \{+P \cdot \sin\overline{|nt-C|} \quad + Q \cdot \sin\overline{|nt-2Nt+C|} \quad + R \cdot \sin\overline{|3nt-2Nt-C|} \quad + S \cdot \sin\overline{|3nt-4Nt+C|}\} \\ +\{+\cdot000000+\cdot0278 \cdot \sin\overline{|2nt-2Nt|}\} \\ \qquad \times t \times \{+P \cdot \cos\overline{|nt-C|} + \cdot8504 \cdot Q \cdot \cos\overline{|nt-2Nt+C|} + 2\cdot8504 \cdot R \cdot \cos\overline{|3nt-2Nt-C|} + 2\cdot7008 \cdot S \cdot \cos\overline{|3nt-4Nt+C|}\} \\ +\{+\cdot999545-\cdot0075 \cdot \cos\overline{|2nt-2Nt|}\} \\ \qquad \times t \times \{-P \cdot \sin\overline{|nt-C|} - \cdot7231 \cdot Q \cdot \sin\overline{|nt-2Nt+C|} - 8\cdot1247 \cdot R \cdot \sin\overline{|3nt-2Nt-C|} - 7\cdot2944 \cdot S \cdot \sin\overline{|3nt-4Nt+C|}\} \end{array}\right\}.$$

On performing the multiplications in these lines, omitting the products of small terms of the first factors by the coefficients of R and S in the second factors, the Second Side of the Factorial Equation becomes

$$+\left\{\begin{array}{l}+\cdot008400 \cdot P \\ -\cdot0123 \quad \cdot Q\end{array}\right\} t \cdot \sin\overline{|nt-C|} \qquad +\left\{\begin{array}{l}-\cdot0114 \cdot P \\ +\cdot2851 \cdot Q\end{array}\right\} t \cdot \sin\overline{|nt-2Nt+C|}$$

$$+\left\{\begin{array}{l}+\cdot0392 \cdot P \\ -7\cdot1130 \cdot R\end{array}\right\} t \cdot \sin\overline{|3nt-2Nt-C|} \qquad +\left\{\begin{array}{l}+\cdot0359 \cdot Q \\ -6\cdot2832 \cdot S\end{array}\right\} t \cdot \sin\overline{|3nt-4Nt+C|}.$$

This is to be compared with the First Side of the Factorial Equation.

Solving the four equations formed by comparison of the coefficients of sines of similar arguments,

$$P = +B \times 1{\cdot}003, \qquad Q = +B \times {\cdot}011, \qquad R = +B \times {\cdot}005, \qquad S \text{ is insensible.}$$

These, it will be remembered, are the important part of the result of the investigation as giving those term-coefficients which increase with time.

We now proceed with the less important terms whose coefficients do not increase with time, but which represent perturbations recurring with similar form and magnitude at every recurrence of similar positions of the Sun and Moon.

On the First Side of the Factorial Equation there are no coefficients independent of t. And there is no doubt that, in general, the terms of the result for constant coefficients will be so small that the retention of the principal part of each factor on the second side will be sufficiently accurate. There is one exception to this; that for each argument there are two coefficients, whose difference enters importantly, as divisor, into the equation; and in the special instance of the coefficients of cos $\overline{|nt-C|}$, the magnitudes of those coefficients are so nearly equal, and their difference (or divisor of the equation) so small, that in order to produce an available result, a greater number of decimals must be retained in their values. Remembering that the terms multiplying t have been entirely cleared out, the equations now to be treated are contained in these two lines—

$$0 = \left\{ \begin{array}{r} +1{\cdot}007945 \,.\, p \,.\, \cos \overline{|nt-C|} \qquad + q) \,.\, \cos \overline{|nt-2Nt+C|} \\ + \ {\cdot}999545 \,.\, (2P-p) \,.\, \cos \overline{|nt-C|} + (1{\cdot}7008 \,.\, Q - {\cdot}7231 \,.\, q) \,.\, \cos \overline{|nt-2Nt+C|} \\ + r \,.\, \cos \overline{|3nt-2Nt-C|} \qquad + S \,.\, \cos \overline{|3nt-4Nt+C|} \\ + (5{\cdot}7008 \,.\, R - 8{\cdot}1247 \,.\, r) \,.\, \cos \overline{|3nt-2Nt-C|} + (5{\cdot}4016 \,.\, S - 7{\cdot}2944 \,.\, s) \,.\, \cos \overline{|3nt-4Nt+C|} \end{array} \right\}.$$

Referring to the late investigations of the values of R and S, it will be seen that r and s are totally unimportant; even q is far too small to be recognised in observation. But p is more important. Its equation is

$-1\cdot999090.\mathrm{P}+\cdot008400$. $p=0$, or $p=-238\times\mathrm{P}$, which is sensibly equal to $-238\times\mathrm{B}$, or to $-238\times\cdot479\times\sin 1''\times\frac{1}{84}$; or in seconds of arc, $p=-1''\cdot36$.

The physical result of the investigation, as regards the terms whose coefficients depend on time, may be thus presented. The elevation above the fixed ecliptic of 1900, of that part of the moveable ecliptic of $1900+t$, near which the Moon happens to be at the time t, is $\mathrm{B}t.\sin\lfloor v-\mathrm{C}\rfloor$, or

$$\mathrm{B}t\times\{+\sin\lfloor nt-\mathrm{C}\rfloor+\cdot00574\,.\,\sin\lfloor nt-2\mathrm{N}t+\mathrm{C}\rfloor+\cdot00574\sin\lfloor 3nt-2\mathrm{N}t-\mathrm{C}\rfloor\}.$$

Thus we have

Perturbation of the Moon at the time t, above the fixed ecliptic of 1900. $=\mathrm{B}t\times\{+1\cdot003\,.\,\sin\lfloor nt-\mathrm{C}\rfloor+\cdot011\,.\,\sin\lfloor nt-2\mathrm{N}t+\mathrm{C}\rfloor+\cdot005\,.\,\sin\lfloor 3nt-2\mathrm{N}t-\mathrm{C}\rfloor\}$,

Elevation above the fixed ecliptic of 1900, of that part of the moveable ecliptic of $(1900+t)$ near which the Moon happens to be at the time t. $=\mathrm{B}t\times\{+\sin\lfloor nt-\mathrm{C}\rfloor+\cdot006\,.\,\sin\lfloor nt-2\mathrm{N}t+\mathrm{C}\rfloor+\cdot005\,.\,\sin\lfloor 3nt-2nt-\mathrm{C}\rfloor\}$,

Excess of the first line or Perturbation of the Moon at the time t above the moveable ecliptic at the time t. $=\mathrm{B}t\times\{+\cdot003\,.\,\sin\lfloor nt-\mathrm{C}\rfloor+\cdot005\,.\,\sin\lfloor nt-2\mathrm{N}t+\mathrm{C}\rfloor\}$.

The last figures I believe to be purely accidental, arising from the use of decimals to a point almost beyond what they can bear. Rejecting them, the general result is—

(A.) The Moon's movements, in so far as they might be supposed to depend on coefficients increasing with time, are the same with reference to a disturbed ecliptic as they would be with reference to an undisturbed ecliptic. (The immediate relative action of planets upon the Earth and Moon is not considered.)

But there is also a term $-1''\cdot36 \cos \overline{|nt-C|}$ in the Moon's latitude, with invariable coefficient, which may be expressed in the form

$$-1''\cdot36 \,.\, \sin \overline{|\text{Moon's longitude} - 82^\circ\ 50'|}$$

or

$$+1''\cdot36 \,.\, \sin \overline{|\text{Moon's longitude} - 262^\circ\ 50'|}$$

Comparing this with the assumptions at the beginning, we see that—

(B.) The movements of the Moon, as expressed by the ordinary theories, are to be referred, not to the moveable ecliptic, but to a plane which is inclined to the moveable ecliptic in an angle of $1''\cdot36$; the axis of inclination being at right angles to the axes of rotation of the moveable ecliptic on the fixed ecliptic, and the greatest depression taking place at longitude $172''\ 50'$.*

We may express the term

$$-1''\cdot36 \,.\, \cos \overline{|\text{Moon's longitude} - 172^\circ\ 50'|}$$

under the equivalent form

$$+1''\cdot35 \,.\, \cos \text{Moon's longitude} - 0''\cdot17 \,.\, \sin \text{Moon's longitude}.$$

Now, on referring to the discussion of the Greenwich lunar observations in the *Memoirs* of this Society, volume xxix. page 23, it will be seen that the observations give an equation in latitude

$$+1''\cdot93 \,.\, \cos \text{Moon's longitude} - 0''\cdot59 \,.\, \sin \text{Moon's longitude},$$

(an equation which was the source of anxious consideration to Professor Hansen and myself). I do not doubt that this equation is completely explained by the result of the preceding theory. The law of connection of the two terms is well in accord; the magnitude of the coefficients is not perfectly in agreement, but not so discordant as at first appears (since the coefficients from the observations are the results of observed coefficients, every one of which is far inferior in merit to the exhibited result). It may be well to examine severely the factors in the investigation cited above.

1881, *February* 1.

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* In the *Darlegung*, p. 118, Hansen arrives at the conclusion that, disregarding periodic inequalities, the Moon moves in an orbit having a constant inclination, "nicht auf der gleichzeitig stattfindenden Ecliptik, sondern auf der welche nahe drei Jahre vorher Statt fand, und deren Knoten mit der gleichzeitigen Ecliptik überdies um einen Quadranten zurück gewichen sind." See also p. 494 of the *Darlegung*, where reference is made to the equation in latitude obtained from the Greenwich Lunar Observations.—Ed.

A Reply to Mr. Christie's Paper "On Mr. Stone's Alterations of Bessel's Refractions." By E. J. Stone, Esq.

In the *Monthly Notices* of the Society, vol. xli. No. 3, January 1881, there appears a paper by one of our Secretaries, Mr. Christie, which renders a reply desirable.

I am of opinion that good, and not harm, results from the full discussion of differences of opinion on scientific points; but the facts must not be obviously strained on either side. I should not have considered it necessary to reply to Mr. Christie were it not that some statements, on page 95, have been seriously misunderstood by many readers; and that some others, on page 107, appear to me to be deficient in precision.

In my paper on Mean Refractions, vol. xl., No. 6, 1880 April, I brought forward—at the request of some Fellows of the Society—the result of a recomputation of the residual error for the group of stars, 43° to 46° 29′, which was originally given in my paper on Bessel's Mean Refractions in the *Monthly Notices* for November 1867. The following statements are extracted from my paper of April 1880, pp. 338 and 339:—

"It may probably avoid useless discussion if I state clearly all the corrections applied to the Greenwich results.

"First: The observations have been extracted from the Annual Catalogues of the Greenwich Observatory 1857 to 1865 inclusive.

"Secondly: I have not used the Reflexion Observations. These observations have been already combined with the direct observations in the determination of the Nadir Points and in the deduction of the R—D corrections, and the corrections thus deduced have been applied in the Greenwich volumes to the direct results. With this exception, all the results given in the Greenwich volumes have been used.

"Thirdly: I have assumed that the thermometer used in 1857 and part of 1858 read too high by six-tenths of a degree Fahrenheit.

"Fourthly: I have assumed that the barometer used in the reductions read too low by a hundredth of an inch.

"Fifthly: As some of the observations contained in the group (5) have a greater zenith distance than 82°, and these observations have had their refractions already decreased by $0{\cdot}0023 \times$ tabular refraction, I have therefore increased their refractions by the same amount to bring them back to the results which would have been obtained had Bessel's Refractions been used unaltered.

"Sixthly: The observations have all been reduced to an assumed colatitude 38° 31′ 21″·80.

"Seventhly: I have weighted the observations from the formula—

$$W = \frac{1}{2\epsilon^2 + \frac{e^2}{n} + \frac{e'^2}{n_,}}$$

assuming $2\epsilon^2 = 0''.04$, and extracting e and e' from my paper on Probable Errors in the *Monthly Notices* for 1869. The result obtained in 1867 was $2''.19$ with a weight 28, or probable error $\pm 0''.2$. The result which I now obtain is $2''.10$ with a weight 24, or probable error of $\pm 0''.2$. The two results are essentially identical, and it is clear that all the corrections now applied were applied in 1867."

I am unable to appreciate the grounds upon which Mr. Christie can ever have supposed that, in a discussion regarding the necessity for certain breaks of continuity in Bessel's Refraction adopted at Greenwich, I carried on that investigation without taking these breaks of continuity into consideration. But I certainly thought that the statements contained in my paper of April 1880 were sufficiently explicit. It appears, however, that I was mistaken, for on pp. 95 and 96 of Mr. Christie's paper in the *Notices* for January 1881, we may read as follows—

"Notwithstanding Mr. Stone's explicit statement in the *Monthly Notices* for April 1880, pp. 338, 339, of the corrections which he has applied in forming his last residual, there are several points to be cleared up—

"1. Corrections for error of thermometer and barometer are stated to have been applied to the observations in 1857 and part of 1858. Now, I do not find in the Greenwich 'Introductions' any observations in those two years which would fall in this group.

"2. The limits of Mr. Stone's last group are N.P.D. 43° to 46° 29′. I have not been able to find any observations in the Greenwich 'Introductions' between N.P.D. $45\frac{1}{2}°$ and $47\frac{1}{2}°$.

"3. The assumed co-latitude is stated to be 38° 31′ 21″·80, but in his paper of 1867 Mr. Stone, after applying a correction $-0''.205$, obtains 38° 31′ 21″·61, which would imply an assumed co-latitude 38° 31′ 21″·815. The difference is trifling, but it must be remembered that Mr. Stone gives his residuals to a thousandth of a second.

"I call attention to (1) and (2), not in any spirit of criticism, but because there seems a possibility that as Mr. Stone extracted his results from the Greenwich Annual Catalogues, his last group may contain observations accidentally omitted from the Latitude Investigations in the Greenwich 'Introductions' which I used.†"

[Note, page 95: "† On examining the Greenwich Observations for 1857, I find that there are, as a matter of fact, several stars in the Annual Catalogue which, for some reason, are not included in the Latitude Investigation in the Introduction. Mr. Stone had not stated in his paper of 1867 that he had taken his

results from the Annual Catalogues, and I thought it more natural that he should have taken them from the Introductions where they are collected. Thus our data are not the same."]

But in continuation we find, page 96—

"As regards the residuals, there is a point of some importance on which I am at direct issue with Mr. Stone—viz. the estimated probable error of the result for the last group. Mr. Stone obtains a weight of 28 or 24 with a probable error $0''{\cdot}2$, while with the same system of weights I obtain a weight 6·5, with a probable error of $0''{\cdot}4$. I have for the last group taken the probable error of an observation above the pole as $0''{\cdot}5$ and S.P. as $1''{\cdot}5$, and I have altogether 30 observations above pole and 17 below, which gives as an *inferior* limit to the probable error $0''{\cdot}38$, on the supposition that they all referred to the same star. It is just possible that the additional observations included by Mr. Stone may account for the discrepancy.

"In any case there are, I believe, but few stars observed in this group; and where the same star—*Capella*, for instance—is observed every year on the same division of the circle, account ought to be taken of the probability of a systematic error affecting the series. Thus the weight ought not to be assumed to be the sum of the weights for the separate years. Unless Mr. Stone has followed this last and erroneous system, I hardly see how he can have got such a large weight and small probable error for the result of his group."

I do not profess to understand the real drift of these statements of Mr. Christie. But there is an uncomfortable look about the combination of this pointed reference to pp. 338, 339, of my paper in the *Notices* for April 1880, and Mr. Christie's paragraphs (1) and (2), which has misled many readers, and conveyed an impression which I am sure that Mr. Christie could never have intended.

It is clear, from the note given at the bottom of page 95, that Mr. Christie is quite aware that he has overlooked a number of Greenwich observations which have an important bearing on these questions of refraction. As a matter of fact, there are in the Greenwich volume for 1857 no less than seven different stars observed, which fall within the group N.P.D. 43°—46° 29′, with 30 observations above the pole and 28 below the pole, whose results are not given in the Latitude Investigation in the Introduction. It appears now that these observations, and some others, have been omitted by Mr. Christie from his discussions. It is clear, therefore, that all force is removed from the paragraphs (1) and (2), which prove no error or misstatement whatever on my part; and it is equally clear that Mr. Christie was not likely to obtain as small a probable error for the group as that obtained by me, because he had not made use of *one half of the materials available for the discussion.*

I greatly regret that Mr. Christie has thought it necessary to publish criticisms which appear to me to derive whatever force

they may have, in the minds of his readers, from misconceptions of his meaning and intention.

I now come to the consideration of the statements made by Mr. Christie on page 97.

Mr. Christie considers my statement, "that the accumulated evidence of the Greenwich circumpolar observations is against the accuracy of Bessel's Refractions unaltered," as "really quite misleading." If I thought that such was the case the statement should be at once withdrawn; but the statement appears to me perfectly correct after the best reconsideration which I can give it. In 1857 Mr. Main discussed the Greenwich observations (below 70° zenith distance) made during the years 1836 to 1854, both inclusive. The practical result of that discussion was that Bessel's Refractions were abandoned at, and below, 82° Z.D. and another set of Tables=Bessel's Tables (1−0·0023) used for zenith distances between 82° and 85°. At 85° this second set of Tables was replaced by a third=Bessel's (1−0·0049). I presume, therefore, that the Greenwich observations 1836 to 1854 are not represented within probable errors by Bessel's Refractions unaltered, or these large and arbitrary changes would not have been considered necessary in the reduction of the Greenwich observations.

My investigation, made in 1867, discusses the observations made at Greenwich 1857 to 1865. The residual errors for five groups with N.P.D—0° to 15°; 15° to 30°; 30° to 37° 40′; 37° 40′ to 43°; and 43° to 46° 29′—are as follows:—

	(1)	(2)	(3)	(4)	(5)
With Bessel's Refractions, A	+0″·240	+0″·196	+0″·570	+1″·054	+2″·190
With Bessel's (1−0·0053) R. B	+0·15	−0·10	−0·14	−0·08	+0·10
The weights are	130	88	31	54	28

From 1868 to 1876 the Greenwich observations were reduced with Bessel's Refr. (1−0·0053), and the residual errors, with the colatitude determined, as it should be, from the observations themselves, but without attributing any weight whatever to the two last groups, which are chiefly affected by errors in the adopted refractions, are as follows:—

	(1)	(2)	(3)	(4)	(5)
C	+0″·15	−0″·22	+0″·05	−0″·31	+0″·22.

In this case the last two groups contain observations to 85° and below 85° respectively. Now, I have no hesitation in saying, no practical astronomer with the residual errors B and C before him, and informed that they contain the excess of N.P.D. below pole over N.P.D. above pole, for groups extending, in the case

of B to 85° Z.D., and in the case of C to below 85° Z.D., would, if in ignorance of what refraction tables had been used, express any other opinion than that the agreement was remarkably close, and proved the substantial accuracy of the mean refractions adopted. These are the facts upon which my statement was based, and I cannot recall it. But Mr. Christie refers to his paper (*Memoirs*, vol. xlv. p. 161) as affording some evidence against these diminished refractions and in favour of Bessel's Refractions unaltered. Let us see what this evidence amounts to. Of the four periods discussed by Mr. Christie, the observations made 1840–1847 are not extended below zenith distance 78°, and in such a discussion Mr. Christie was hardly likely to obtain any positive evidence against the refractions adopted for the determination of the colatitude. With respect to the periods 1851–1861, 1862–1865, Mr. Christie has unfortunately grouped together observations reduced with Bessel's Refractions and observations reduced with refractions obtained by throwing away from Bessel's Refractions quantities varying from 0″·9 to over 2″. And it now appears that he has omitted from the discussion altogether a large portion of the most valuable material, valuable so far as questions of refraction are concerned, which is available in the Greenwich volumes. I consider, therefore, that Mr. Christie's work requires revision throughout.

And here I must call attention to what, after giving the matter a careful consideration, appears to me a serious fallacy which invalidates Mr. Christie's work upon these questions of refractions. The refractions which he uses for comparison with the results obtained, 1868 to 1876, are not in reality Bessel's Refractions. They are Bessel's Refractions unaltered down to 82° zenith distance. At that zenith distance the error of Bessel's Refractions has apparently become sufficiently large to be distinctly put in evidence, and to be separated from the systematic instrumental errors to which all astronomical results are liable. At 82° a quantity of about 0″·9 is thrown away from Bessel's Refractions, and a new set of Tables used from 82° to 85° Z.D. At 85° Z.D. the error has apparently again become sufficiently large to be put in evidence, and the refractions are at 85° Z.D. a second time arbitrarily diminished by about 2″·9 from Bessel's Refractions, and a third set of tables used below 85° Z.D. Need I point out that the more conclusively it can be shown that the observations are represented by refractions thus computed, the more certain it is that *Bessel's Refractions unaltered* do not and cannot represent the Greenwich observations.

I know nothing about the errors of Bessel's Refractions except what I can gather from the observations. If these large corrections and abrupt breaks at 82° and 85° are not required, they should not be applied; but if they are required, it is useless to contend that Bessel's Refractions unaltered represent the Greenwich observations. And surely it is unnecessary for me to point out, if Bessel's Refractions are too great by 0″·9

at 82° Z.D. and the error is increasing in the same direction to 85° Z.D., that Bessel's Refractions cannot be without sensible error at zenith distances less than 82°. The error cannot change *per saltum* at 82°.

If the errors of Bessel's Refractions are such quantities as those given in my paper of April 1880, viz. +0″·4 at 70°, 0″·16 at 76°, 0″·28 at 78°, they are hardly likely to be detected from a general discussion of observations limited to an arc of 78°, when the observations are affected, as all the observations with which I am acquainted are affected, with systematic instrumental errors of at least equal or larger amounts.

With respect to what Mr. Christie, on page 104, calls my "*tinkering*" of Bessel's theory. What I have done is as follows:— Bessel's theory has two constants to be determined from observation. He first tried to include the refractions from the zenith to the horizon under one general investigation, and the errors of his theory were not, as a matter of fact, very great. But it was clear to Bessel that his refractions were too large at 85° by about 2″; and he recommended that for greater zenith distances than 85° the results of his theory should be replaced by refractions deduced more directly from observation. Now in the *Notices* for April 1880, I pointed out, and I think that any one who has had much experience in such investigations will recognise the importance of the point, that when Bessel found it necessary to abandon his attempt to include all the refractions from the zenith to the horizon in one investigation, and restricted the theory to a smaller arc than 90°, he should have re-determined both the constants of his theory from observations included within the restricted arc, and not allowed one of the constants to be strained by attempts to represent observations near the horizon, attempts which had confessedly not been successful.

What I think Bessel should have done when he found his refractions too large at 85° Z.D., I have done, and the results are given in the *Notices* for April 1880. I have, as there clearly stated, assumed the error at 85° Z.D.=2″·87. This quantity may be, for aught I know, too large from the accidental run of the systematic errors of the Greenwich observations. But I have no good reasons for supposing such to be the case. If the assumed error be sensibly correct, then the corrections given by me for Bessel's Refractions at smaller zenith distances are entitled to be received with confidence; but if the error 2″·87 is much too large for Z.D. 85°, then the errors given for the smaller zenith distances will also be slightly too large. If this be *astronomical* "*tinkering*," all I can say is, that I am well content to be an "Astronomical Tinker."

With respect to Mr. Christie's objections to my use of the co-latitude determined from the observations 1868–1876 in exhibiting the residual errors of these observations, I am quite unable to follow Mr. Christie's reasoning. If the method, adopted at Greenwich 1871–1875, of applying a correction of 0″·6 to

0″·8 to the nadir determinations before using them, in combination with the reflexion observations, for the determination of zenith points was perfectly satisfactory, then the co-latitudes determined from the observations should be perfectly satisfactory, and should be used, as I have done, and not rejected, as Mr. Christie has done. If this method of treating the not inconsiderable discordance between the nadir points, although the best practical method of meeting a confessed difficulty, is not altogether satisfactory, then the use of the co-latitudes determined from the observations themselves should, to a great extent at all events, correct any outstanding constant error; but, to secure this, the co-latitude deduced from the observations which are affected with the same constant error must be used. Mr. Christie's argument, on page 108, although brought forward to prove me wrong, affords, in my judgment, the most satisfactory evidence that could be adduced in favour of the course which I have adopted. "The apparent change in the co-latitude on which Mr. Stone remarks has nothing to do with this correction (*i.e.* constant difference between the nadir points with reflecting eyepiece and reflexion stars), but is simply due to a change in the system of weights given to the close circumpolar stars and to the increased number of observations of wide circumpolars. It did not begin in 1871, when the correction to the nadir observation was first applied, but in 1872, when special attention was directed to the observation of wide circumpolars. Previous to that year the large weight given to the four close circumpolars made the value of the co-latitude virtually depend on them; and if this system had been continued the same value would have been obtained in subsequent years. The four close circumpolars give for the period 1868–1876, 38° 31′ 21″·95 with Bessel's Refractions, and 38° 31′ 21″·70 with the diminished refractions; whereas the stars about N.P.D. 20° give about 38° 31′ 21″·50 and 38° 31′ 21″·17. It is simply the admission of these latter stars to a fair share in the result which has produced an apparent change. The apparent change does not justify Mr. Stone in using two different co-latitudes in the periods 1857–1865, and 1868–1876." Surely, if the smaller co-latitude given by these observations, 1868–1876, is due solely to a better distribution of the weights than that formerly adopted, Mr. Stone is, at all events, justified in using the co-latitude deduced from these observations, and Mr. Christie is certainly not justified in rejecting it in favour of a value which would have been obtained with a more imperfect system of weights. But we are really concerned in these questions with the effective co-latitude (*i.e.* the actual differences of readings on the circles between the mean adopted nadir point, or zenith point, and the polar point) and not with the real colatitude of the Greenwich Observatory, even should there, as a matter of fact, be any sensible difference between them.

I must now refer to some statements of fact by Mr. Christie,

on page 107, which I do not think are made with the precision which is desirable.

"In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^r of the microscope micrometers, and thus was obtained that accordance of separate results which is so dear to some astronomers, but which is not always conducive to the elimination of systematic errors. The additional wear resulting from this increased use of one part of the screws gave rise to a discordance between the nadir observations and observations of stars which were taken at all parts of the screws. This discordance is shown in the years 1869 and 1870 by the large constant term in R−D; in subseqnent years a correction was applied to each nadir observation derived from the mean results for the preceding year. In 1875, being dissatisfied with the large correction for runs virtually required to reduce the nadir observation to the mean of observations of stars which would fall at about $2^r{\cdot}5$, I suggested that the nadir observation should be taken at from $2^r{\cdot}5$ to $3^r{\cdot}5$, and it was then found that the correction to the nadir observation had changed from $-0''{\cdot}8$ to $+0''{\cdot}6$. This threw suspicion on the micrometer screws, and their errors were examined, as explained in my paper of 1876. The result was to show that corrections were required ranging from $-1''{\cdot}4$ at $\bar{1}^r{\cdot}3$ to $+0''{\cdot}8$ at $2^r{\cdot}3$, the correction at $0^r{\cdot}0$ being $-0''{\cdot}9$, corresponding very closely with the correction to the nadir observation $-0''{\cdot}8$. Further, observations taken for another purpose in 1868 showed that the errors of the micrometer screws were sensibly the same at that time, with the exception of the portion about $0^r{\cdot}0$, which had suffered extra wear since 1868 from a preponderance of some 4,000 observations taken at this part."

If this statement means that in 1868 the practice was introduced of taking the nadir points near the readings $0^r{\cdot}0$ of microscope A, then it is certainly not correct. If it simply means that, to avoid very large corrections for run, the nadir points were generally taken, after 1868 Feb. 1, at readings between $0^r{\cdot}0$ and $1^r{\cdot}5$, or over a range of the screws with which more than a quarter of all the observations made with the circle would be made, then the statement may be accepted as substantially accurate. But, in this case, it will fail to account for the serious wear of the screws near $0^r{\cdot}0$, which Mr. Christie supposes to have taken place shortly after the introduction of the change. I have taken the trouble to examine the Greenwich observations 1868 and 1869, and I find as follows. From 1868 Feb. 1 to 1869 Dec. 31 there were only 108 determinations of the nadir point at readings of microscope A less than $0^r{\cdot}2$, whilst there were 72 determinations made at readings of microscope A above $1^r{\cdot}0$. The largest reading is $3^r{\cdot}0$, but there are several as large as 2^r. Whatever, therefore, may have been the cause of the state of the screws near $0^r{\cdot}0$ in 1876, it certainly was not caused by the additional 4,000 observations made near $0^r{\cdot}0$ at my suggestion.

I left for the Cape in 1870. The last discussion of the R−D which I could have had the advantage of seeing was that for 1868. But if the screws were worn, in the way indicated by Mr. Christie in his paper of 1876 November, so that the error changed from $-1''\cdot2$ at $-\overset{r}{1}\cdot9$ to $-0''\cdot9$ at $0^r\cdot0$ and to $-0''\cdot5$ at $0^r\cdot1$, then the values of the *runs* determined for readings somewhere near $0^r\cdot0$ to $4^r\cdot8$ or $4^r\cdot9$ must have been seriously affected, and have depended very much indeed upon the exact reading from which the run was actually commenced. From the form of the curve of errors given in Mr. Christie's paper 1876, I should have supposed that the runs had not been corrected for errors of screws before their application, and that this oversight had caused the errors of the screws to be *apparently* nearly the same for the large readings near $4^r\cdot8$ and for the small readings, from which the run was generally started; but I am persuaded that such an oversight could not possibly have been made by Mr. Christie in a work of importance.

The correction of the Greenwich Results for these assumed errors of screws cannot be completed without a correction of the runs, of the readings of the screws employed in making any particular observation, and of the readings included in the adopted zenith point determination; and as the actual readings from which the runs are determined are not published, I have been unable to correct the Greenwich Results for errors of screws. But fortunately such corrections are certain, in mean results, to be very small; and in questions of errors of our refraction tables at such zenith distances as 82° to 85° and 85° to 87° with which I was concerned, such errors are altogether unimportant.

As discussions involving in any degree personal questions have a tendency to obscure the real points at issue, and as I have neither the time nor inclination for mere verbal discussions, I will state, as clearly as I can, what I maintain and what I admit with respect to these questions of refraction.

1. I maintain that the Greenwich circumpolar observations are not represented within errors of observation by *Bessel's Refractions unaltered* from 0° to 85° Z.D. Bessel himself recommended their abandonment below 85° Z.D.

2. I maintain that Bessel's Refractions $(1-0\cdot005)$ do, whether accidentally or not I cannot say, represent, within errors of observation, the Greenwich circumpolar observations 1857 to 1876.

The co-latitude must of course be determined with the same set of refraction tables.

3. I infer, from the general results of Main's discussion of the observations 1836–1854, that if these observations be treated *as a whole*, and no abrupt breaks in continuity of the refractions used be admitted, then they would be represented either within, or very nearly within, errors of observation, by the use of Bessel's Refractions $(1-0\cdot005)$. This I think is shown in my paper of April 1880.

4. I point out that the observations 1836–1854 and the observations made 1857 to 1876 at greater zenith distances than 85° are closely represented also by these diminished refractions.

5. I freely admit that a comparison between the Cape observations, which I have recently put into the hands of astronomers, exhibits a most astonishingly close agreement, for moderate zenith distances, with the Greenwich Results when the observations are reduced with Bessel's Refractions unaltered; but the observations show that Bessel's Refractions at 82°, and certainly at 85°, are much too large for the Greenwich observations.

6. I maintain that any refraction tables, to merit acceptance, should satisfy the known conditions without any abrupt breaks of continuity over the arc for which they are used.

7. I fully admit that the solution obtained by me in 1867, which satisfied the then known conditions, does not satisfy *all* the conditions brought forward by my Cape work.

8. I maintain that Bessel's Refractions unaltered fail to represent either of the necessary conditions to 85° Z.D.

9. I believe that by a slight but necessary correction of Bessel's theory, brought forward in the *Notices* for April 1880, the conditions, so far as I know them at present, are fulfilled.

I therefore, of course, much prefer the solution of 1880 April to that of 1867 November. The one satisfies one set of conditions, whilst the other satisfies two. A complete solution must satisfy all the conditions of a problem. A solution to be worth acceptance even for a time, and until replaced by something better, should satisfy all the known conditions. But, if it does satisfy the known conditions, it is worth acceptance until facts inconsistent with it have been distinctly put in evidence. It must then, of course, be modified or abandoned. I know of no method by which progress, and not stagnation, can be secured, except by thus testing and modifying our theories as fresh data are brought forward.

What I have objected to, and do most strongly object to, is that directly some fresh conditions for testing these questions of refraction were brought forward by my publication of the Cape Catalogue 1860, some astronomers appeared to rush to the conclusion that these conditions were alone worth consideration, and that all our previous work was wrong; and (overlooking, or at least not giving any sufficient prominence to, the fact that the Greenwich observations, which agreed so well with the Cape observations, agreed at considerable zenith distances only by a virtual abandonment of Bessel's Refractions) they upheld the accuracy of Bessel's Refractions unaltered, and attempted to explain away the force of the evidence slowly accumulated at Greenwich by the labour of years; and, whether intentionally or not, certainly created a false impression that I had committed some serious mistake in my paper of 1867. No mistake, so far as I am aware of, was made; the whole of the available data were represented by the solution obtained.

Note on a Paper by Mr. Denning in Monthly Notices, No. 2, vol. xli. By J. L. McCance, Esq.

Mr. W. F. Denning has a paper on "*Saturn* in October 1880" in the *Monthly Notices*, vol. xli. No. 2. I only wish to say a very few words with reference to one subject, namely, the "curious figure" of the planet which Mr. Denning describes. I beg to corroborate his remarks as to this odd appearance. I have observed it on several occasions, and ascribed it much to the same causes as does Mr. Denning. It follows, I imagine, that this "square-shouldered" aspect must be more or less prominent as the arrangement of the belts varies from year to year; and this may be the reason of the observations of it being comparatively rare. It would be interesting to learn if other observers have noticed this appearance near the late Opposition; for myself I did not observe *Saturn* near the Opposition, but I have a Note on Nov. 21, $11^h\ 20^m$, that the "square-shouldered" aspect was very plain then.

Putney Hill, S.W.
1881, *Jan.* 24.

Comparative Observations of Disappearance and Reappearance of Jupiter's Satellites. By the Rev. S. J. Johnson.

The following observations during the late apparition of *Jupiter* were taken with a $3\frac{1}{4}$ in. and $2\frac{1}{4}$ in. telescope placed side by side, each charged with a power of 100, for the purpose of noting the difference caused by one inch of aperture in the earliest and latest visibility of the eclipsed satellites. The time was obtained by three or four single altitudes of the Sun taken with the sextant. The immersions and emersions, as might be expected with a small instrument, correspond, in the case of the first satellite, pretty closely with the predicted times given in the "Nautical Almanack;" the result of observations on Nov. 4, 27, and Dec. 20 according with the latter within 4 sec., 2 sec., and 1 sec. respectively.

1880, Sept. 3	III.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	11	16 28
			$3\frac{1}{4}$ „	11	17 52
7	II.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	11	11 48
			$3\frac{1}{4}$ „	11	12 39
8	I.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	13	15 28
			$3\frac{1}{4}$ „		

Planet in slight fog. On removing my eye to the larger instrument, I found it was not in sight.

Nov. 3	II.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	10	28	2
			$2\frac{1}{4}$ „	10	28	39
Nov. 4	I.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	6	46	39
			$2\frac{1}{4}$ „	6	46	55
Nov. 20	I.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	5	7	19
			$2\frac{1}{4}$ „	5	7	29
Nov. 27	I.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	7	2	58
			$2\frac{1}{4}$ „	7	3	18
Nov. 28	II.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	7	33	23
			$2\frac{1}{4}$ „	7	33	39
	III.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	11	44	48
			$3\frac{1}{4}$ „	11	45	35

Planet in slight haze.

Dec. 20	I.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	7	19	33
			$2\frac{1}{4}$ „	7	19	48
Dec. 30	II.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	7	16	55
			$2\frac{1}{4}$ „	7	17	13
1881, Jan. 6	II.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	9	54	2
			$2\frac{1}{4}$ „	9	54	32
Jan. 31	II.	Ec. R.	With $3\frac{1}{4}$ inch, first visibility	7	3	29
			$2\frac{1}{4}$ „	7	4	23

Abbenhall Rectory, Gloucester,
1881, *March* 10.

Observations of the Phenomena of the Satellites of Jupiter, with a few Transits of the "Red Spot;" also a few observations of the brighter Satellites of Saturn, made at the Observatory of Mr. Edward Crossley, F.R.A.S. By Mr. J. Gledhill, F.R.A.S.

The instrument used is the equatorial refractor of $9\frac{1}{2}$-in. aperture. In the observations of the phenomena of *Jupiter's* satellites the full aperture and powers 240 and 470 were used. The transits of the "red spot" and the phenomena of *Saturn's* satellites were observed with the Simms micrometer, powers 280 and 460; in the former observations the equatorial diameter of *Jupiter* was measured, the webs set at half the value, and one kept on the eastern or western limb of the planet at right angles to the planet's equator: in the latter a web was placed at right angles to the plane of the ring, and at one of its ends; or the semi-diameter of the ball was found, and the webs placed as in the transits of the "red spot."

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation.	Mean Solar Time from N. A.	Remarks.
			h m s	h m s	
1880, Aug. 31	II.	Oc. R. External contact	12 57 0	12 58 0	Good definition.
Sept. 3	(*a*) III.	Ec. D. Began to fade	11 14 36	11 17 25	Good definition.
		Disappeared	11 18 42		
7	(*b*) II.	Ec. D. Began to fade	11 10 0		Definition bad.
	(*c*)	Half gone	11 11 20	11 11 1	
	(*d*)	Just disappeared	11 12 50		
17	I.	Ec. D. Fading	9 36 0	9 38 47	Much cloud.
		Invisible	9 38 30		
18	I.	Sh. E. Just off	9 8 0	9 9 0	Wind and clouds.
26	I.	Oc. R. External contact	8 31 0	8 31 0	Much motion.
Oct. 2	(*e*) II.	Ec. D.	8 12 53		Definition poor.
	(*f*)		13 53	8 14 6	
	(*g*)		15 38		
	I.	Sh. I. Notch first seen	10 44 0	10 43 0	Bad definition.
	I.	Tr. I. First contact	10 49 0		
		Bisection	10 52 0	10 51 0	
		Inner contact	10 53 0		
	II.	Oc. R. Last contact	11 5 0	11 5 0	
Oct. 3	(*h*) I.	Ec. D. Disappearance	7 57 6	7 57 26	

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Oct. 3	(*i*) I.	Oc. R. First seen	10 12 0		
		Half off	10 12 30	10 15 0	
		Ext. contact	10 14 30		
9	(*j*) III.	Ec. R. First seen	9 45 12	9 50 19	Good definition.
	(*k*) II.	Oc. D. First contact	10 40 0		
	(*l*)	Bisected	10 42 0	10 40 0	
	(*m*)	Disappearance	10 44 0		
10	(*n*) I.	Oc. D. First contact	9 44 0		
	(*o*)	Bisected	46 30	9 46 0	
	(*p*)	Inner contact	48 0		
19	(*q*) I.	Ec. R. First seen	8 26 28	8 26 53	Good definition.
Nov. 1	II.	Tr. I. First contact	11 58 0		Good definition.
	(*r*)	Bisected	11 59 0	12 0 0	
		Second contact	12 0 0		
	(*s*) I.	Tr. I. First contact	12 14 0		
		Second contact	12 16 0	12 13 0	
3	II.	Oc. D. First contact	6 28 0	6 30 0	Very bad sky.
		Disappearance	6 34 0		
	I.	Tr. I. First contact	6 37 0	6 39 0	Violent boiling.
		Second contact	6 44 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Nov. 3	III.	Tr. I. First contact	6 51 0	6 54 0	
		Second contact	7 5 0		
	I.	Sh. I. Just within	7 21 0	7 19 0	
	I.	Tr. E. Inner contact	8 48 30		Good definition now.
		Bisected	8 50 30	8 52 0	Good observations; the last thought the best.
		Outer contact	8 52 0		
	(*t*) III.	Tr. E. Inner contact	9 8 0		Uncertain.
	(*u*)	Bisected	9 15 0	9 21 0	Fairly good.
	(*v*)	Just off	9 22 0		Fairly good.
	I.	Sh. E. Inner contact	9 27 0	9 33 0	Definition not so good.
		Just off	9 32 0		
	III.	Sh. I. Bisected	9 42 0	9 40 0	
		Just within	9 44 0		
	II.	Ec. R. First seen	10 25 30	10 28 43	
	III.	Sh. E. Inner contact	12 5 0		
		Half off	12 8 0	12 11 0	
		Just off	12 9 30		
Nov. 4	I.	Ec. R. First seen	6 46 25	6 46 42	Bad sky.
17	I.	Tr. I. First contact	10 13 0	10 12 0	Good sky; good observation.
		Fully on disk	10 16 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Nov. 18	I.	Oc. D. First contact	7 27 0	7 27 0	Good observation.
		Disappearance	7 29 0		
	I.	Ec. R. First seen	10 38 7	10 38 6	Good observation.
19	I.	Sh. I. Just on	5 40 0	5 39 0	
	II.	Tr. I. First contact	5 50 0	5 54 0	Rather uncertain.
		Bisection	5 53 0		Good.
		Second contact	5 55 0		Good.
	I.	Tr. E. Inner contact	6 47 0		Not so good.
		Bisection	6 50 0	6 53 0	Good.
		Ext. contact	6 53 0		Best observation.
	I.	Sh. E. Inner contact	7 47 0		Good.
		Bisection	7 49 0	7 53 0	Fair.
		Just off	7 51 0		Good.
	II.	Sh. I. Half on	7 54 0	7 53 0	Fair.
			7 56 0		Good.
	II.	Tr. E. Inner contact	8 24 0		Fair.
		Bisection	8 27 0	8 34 0	Good.
		Just off	8 31 0		Good.
	II.	Sh. E. Inner contact	10 28 0	10 33 0	
		Just off	10 31 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N.A. h m s	Remarks.
Nov. 20	I.	Ec. R. First seen	5 7 1	5 7 7	Good observation.
21	II.	Ec. R. First seen	4 57 4	4 58 4	Good.
25	I.	Oc. D. First contact	9 13 0		Good definition.
		Bisection	9 15 0	9 16 0	
		Disappearance	9 17 0		
26	I.	Tr. I. First contact	6 27 0		Good.
			6 29 30	6 27 0	Fair.
			6 31 0		Good.
	I.	Sh. I. Bisection	7 34 0	7 34 0	
		Just within	7 36 0		
	II.	Tr. I. First contact	8 13 0	8 13 0	Clouding.
	II.	Tr. E. Inner contact	10 51 0		
		Half off	10 54 0	11 0 0	
		Just off	10 57 0		
27	I.	Ec. R. First seen	7 2 32	7 2 56	
28	II.	Ec. R. First seen	7 34 20	7 34 3	
	III.	Oc. R. Outer contact	9 29 0	9 35 0	
Dec. 2	I.	Oc. D. First contact	11 4 0		Fair sky.
		Bisection	11 6 0	11 6 0	
		Last seen	11 8 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Dec. 11	I.	Oc. D. First contact	7 25 0	7 26 0	Bad definition.
		Last seen	7 28 0		
	I.	Ec. R. First seen	10 54 43	10 54 43	
12	I.	Tr. E. Just off	6 54 0	6 52 0	Bad sky.
	II.	Oc. D. Just gone	7 37 0	7 36 0	
14	II.	Sh. E. Inner contact	7 46 0	7 50 0	Bad sky.
1881, Jan. 13	II.	Oc. D. Outer contact	7 14 0		Good.
		Half gone	7 16 0	7 14 0	Fairly good.
		Gone	7 18 0		Good.

Notes.

(*a*) Power 227 on the micrometer; probably the light began to fade a little (a few seconds) later than $11^h\ 14^m\ 36^s$.
(*b*) and (*c*) Uncertain. (*d*) Good; power 500. (*e*) Fair. (*f*) Uncertain. (*g*) Good; power 282.
(*h*) The satellite was fully 2^m in disappearing; it was very near the limb of *Jupiter*.
(*i*) Time uncertain. (*j*) The satellite was fully 3^m in attaining its full brightness. (*k*) and (*l*) may be 1^m in error.
(*m*) Good. (*n*) and (*p*) Good. (*o*) Uncertain. (*q*) The satellite was about 4^m in attaining full brightness.
(*r*) Uncertain. (*s*) Not later than this. (*t*) Uncertain. (*u*) and (*v*) Fairly good.

Transits of the "Red Spot" of Jupiter.

1880, Aug. 31.

Preceding end:

h	m	
13	10	not up.
13	15	central?
13	20	past.

Centre:

h	m	
13	40	not up yet.
13	43	central?

Clouds passed and prevented further observation.

1880, Sept. 3. Definition bad.

Preceding end:

h	m	
10	55 ±	judged central.

Centre:

h	m	
11	10	not up yet.
11	14	central.
11	18	certainly past.

Following end:

h	m	
11	38	not quite up.
11	40	central.
11	44	past.

1880, Sept. 18. Wild night: no definition.

Following end:

h	m	
9	0	not up.
9	10	certainly past.

1880, Oct. 2. Definition not good.

Preceding end:

h	m	
9	27	not up.
9	34	just passed.

Centre:

h	m	
9	54	not up.
9	59	central.
10	4	just past.

Following end:

h	m	
10	25	not up.
10	29	central.
10	32	past.

1880, Oct. 9. Good sky.

Preceding end:

h	m	
10	5	not up yet.
10	7	to 10^h 11^m uncertain.
10	13	central.
10	15	past.

Centre:

h	m	
10	40	not up.
10	42	to 10^h 44^m uncertain.
10	47	past.

Following end:

h	m	
11	10	not up.
11	12	not up.
11	13	to 11^h 14^m uncertain.
11	17	certainly past.

1880, Oct 11.

Preceding end:

h	m	
11	47	not up.
11	48	to 11^h 50^m uncertain.
11	52	judged central.
11	55	past.

Centre:

h	m	
12	18	not up.
12	20	to 12^h 23^m uncertain.
12	25	central.
12	27	probably just past.
12	29	certainly past.

Following end:

h	m	
13	17	not up.
13	23	to 13^h 25^m uncertain.
13	27	judged central.
13	30	past.

1880, Oct. 19.

Preceding end:

h	m	
8	23	not up.
8	24	uncertain.
8	25	central?
8	28	central?
8	30	past.

Centre:

h	m	
8	50	not up.
8	53	central?
8	55	central?
8	56	central?
8	58	certainly past.

Following end:

h	m	
9	21	not up.
9	23	to 9^h 25^m central?
9	27	certainly past.

1880, Oct. 21. Definition very bad.

Preceding end:

h	m	
10	5	not up.
10	7	to 10^h 10^m central?
10	12	past.

Centre:

h	m	
10	36	central?

Following end:

h	m	
11	1	to 11^h 3^m central?
11	5	past.

1880, Nov. 19.

Preceding end:

h	m	
8	45	not up yet.
8	50	not up yet.
8	58	central?
9	0	central?
9	3	past.

Centre:

h	m	
9	20	not up.
9	22	to 9^h 24^m uncertain.
9	26	central.
9	30	past.

Following end:

h	m	
9	45	not up.
9	50	up?
9	52	up?
9	55	past.

1880, Nov. 20. Good definition.

Centre:

h	m	
5	13	not up.
5	15	up?
5	16	up?
5	18	central.
5	19	central.
5	20	past.

1880, Nov. 21. Definition not good.

Centre:

h	m	
6	50	not up.
6	55	central?
7	0	central.
7	5	past.

Following end:

h	m	
7	20	probably central.

1880, Dec. 18. Stormy; planet seen at intervals.

Preceding end:

h	m	
Central at 7	50	±.

1881, Jan. 14. Definition good till the centre had passed, then very poor.

Preceding end:

h	m	h	m	
5	10	to 5	15	central; not up at
5	5,	past at 5^h 20^m.		

Centre:

	h	m		h	m
It passed between	5	40	and	5	45.

Following end:

	h	m		h	m
It passed between	6	10	and	6	15.

Observations of Saturn's Satellites.

1880, Sep. 3. Full aperture, $9\frac{1}{3}$ inches, always used. Good definition.

Tethys at inf. conjunction with the following end of the ring:

h	m	
11	55	not yet up.
12	3	on the web at right angles to axis of ring.
12	8	certainly past.

The satellite is steadily visible in the dark field: a little illumination increases the visibility.

The mist thickened and rendered the conjunction with the centre of the ball uncertain. It occurred between 15^h 30^m and 15^h 40^m.

1880, Oct. 23.

Rhea in conjunction (south) with the centre of the ball:

h	m	
9	40	not up.
9	45	uncertain; probably up.
9	50	uncertain; probably up.
10	0	certainly past.

Powers 240 and 470.

1880, Nov. 1.

Rhea in conjunction (south) with centre of ball:

h	m	
10	30	not yet up.
10	32	uncertain; probably up.
10	32	uncertain; probably up.
10	40	just past.

Full red illumination used: good sky.

1880, Nov. 3.

Dione in conjunction (south) with east end of ring:

h	m	
8	50	on the line.
9	0	certainly just past.

Power 240. Definition not good. At 12^h the satellite was an easy object with the aperture ($9\frac{1}{3}$ in.) reduced to 7 in.

Rhea in conjunction (north) with east end of ring:

h	m	
12	5	not up.
12	7	up?
12	10	certainly past.

1880, Nov. 19. Good sky. *Enceladus* was seen steadily at 7 P.M.

Rhea in conjunction (south) with preceding end of the ring:

h	m	
7	15	not up.
7	17	up.
7	20	certainly past.

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*Second Catalogue of Radiant Points of Meteors.**

By Edwin F. Sawyer, Esq.

The following list of Meteoric Radiant Points has been deduced from my observations comprising the recorded paths of about 900 out of a total of 1,156 shooting stars seen during the last two years (1879–80) at Cambridgeport, Mass., U.S. The positions deduced mainly consist of those of well-known meteor systems, although the radiant centres of a few strongly suspected and possibly new showers have been determined. A few positions deduced also serve to confirm meteor showers previously observed but once or twice, and needing such verification. Extreme care has been exercised in deducing the results, a certain weight being attached to each meteor mapped, showing the accuracy of the recorded path on a scale of one to four, and its corresponding value used in determining the different centres of

* First Catalogue published in the *American Journal of Science and Arts*, vol. xvii. June 1879.

radiation. The peculiarities of each individual meteor mapped, such as length of path, velocity, magnitude, whether accompanied by streaks or not, &c., have also been regarded in reducing the results, so that the positions as determined may be considered as accurate as the number of meteors mapped and the character of this class of observations will allow. Observations were taken nearly every fine night in the absence of the moonlight (with the exception of during the months of January and February 1879, and March 1880, when sickness prevented), but were confined principally to the evening hours between 7 and 12 P.M. A few morning watches were, however, maintained in April, August, October, November, and December for the appearance of the well-known meteoric showers active during these months.

The period of watching each evening varied from one to five hours in duration, and amounted to 221 hours in all. The observations were almost wholly confined to the eastern and north-eastern quarters of the sky. The showers have (as during my previous observations) been deduced from the material gathered from short periods of watching, the average duration of the showers being about four days. The results, therefore, give the observed duration and not the true period of each shower, as yet an uncertain element in the majority of showers now catalogued. The following table exhibits, 1st, the days each month on which observations were obtained; 2nd, the number of hours spent in watching; 3rd, the number of meteors recorded; 4th, the number of radiant points deduced; and 5th, the number of meteor tracks employed in determining the radiant centres.

Months and Days.	Hours.	No. of Meteors seen.	No. of Radiant Points deduced.	No. of Meteor Tracks used.
1879, January and February. None ...	0	0	0	0
March 18, 19	2	7	0	0
April 12, 14, 19–23, 26 ...	16	56	6	32
May 8, 9, 11	3	11	0	0
June 8, 13, 17, 18, 21, 23 ...	6	26	1	9
July 12–14, 17, 19	8	26	3	15
August 7, 10–12, 20	10	171	10	71
September 8, 9, 11, 14, 15, 17, 20	11	45	3	27
October 7, 13, 14, 19, 20 ...	13	77	11	54
November 7, 10, 15, 16 ...	11	60	5	31
December 7, 9, 12, 16	11	69	4	43
Total 49 days ...	91	548	44	282

Months and Days.	Hours.	No. of Meteors seen.	No. of Radiant Points deduced.	No. of Meteor Tracks used.
1880, January 2, 5, 7, 8, 10	13	36	2	13
February 1, 6, 7, 8	6	12	2	6
March. None	0	0	0	0
April 5, 7, 8, 12, 30	8	17	0	0
May 2, 6, 8, 10, 11	6	13	0	0
June 29, 30	3	12	2	9
July 4, 7, 28–31	13	99	8	65
August 1, 6, 8, 9, 26, 28, 31 ...	16	172	9	79
September 1–4, 11, 24, 25, 28–30	19	64	5	37
October 1–3, 5, 6, 8, 9, 24, 25, 29, 31	27	113	10	53
November 1, 2, 11, 12, 25, 26, 29	18	68	2	17
December 3	1	2	0	0
Total 63 days ...	130	608	40	279
1879 ... 49 days	91	548	44	282
1880 ... 63 „	130	608	40	279
Grand Total ... 112 days	221	1156	84	561

The estimated magnitudes of the meteors seen were as follows :—

>1st.	=1st.	=2nd.	=3rd.	=4th and fainter.	Total.
76	142	201	280	457	1156

Of the meteors >1st mag. in brightness, there were 3 > ♀, 3= ♀, and 32 equal or exceeding ♃ in brilliancy. 79 of the 218 meteors = or >1st mag. in brightness were noted as exhibiting more or less colour, as follows :—8 were classed as deep orange, 39 orange, 4 yellow, 12 green, and 16 blue. The average length of path of the 912 meteors mapped was 9° 9′. The following list of stationary, or nearly stationary, meteors have been recorded during the past two years :

Stationary Meteors.

No.	Date.	Position.	Mag.	Weight.	Remarks.
1	1879, March 15	195°−5°	3	3	
2	19	198+7½	4	4	Slight movement, about ¼°
3	April 12	223−2	1	3	Very slight movement; visible 1ˢ
4	14	206−8	2	4	
5	14	205−2 to 204−1½	3	4	Path 1°
6	14	207+25	3	3	
7	21	238−7	4	3	
8	21	262+17	4	3	
9	22	253+5	1	3	

No.	Date.	Position.	Mag.	Weight.	Remarks.
10	1879, May 9	275° + 12°	2	3	1 sec.
11	9	225 + 3	1	3	Nearly stationary, path $\frac{1}{4}$°
12	June 8	259 + 11 to 259½ + 10	3	1	Path 1°
13	13	282½ + 20	4	3	
14	23	298 + 27½ to 298 + 26½	4	3	Path 1°
15	July 12	325 + 28	4	3	Slight movement, ½°
16	Oct. 13	11 + 40	4	3	
17	14	22 + 21	2	4	Nearly stationary, yellow.
18	19	18 + 29	5	3	
19	19	92½ + 24½ to 91½ + 24	1	4	Slight movement.
20	Nov. 7	42 + 32	4	3	
21	10	54 + 2	3–5	4	Two seen within the space of 3 seconds from apparently the same point.
22	10	87½ + 19 to 88 + 18	5	3	Slight movement.
23	16	69 + 15	5	4	
24	Dec. 12	117 + 11	> 1	3	
25	12	106 + 24	4	3	
26	12	117 + 24	4	4	Nearly stationary, from direction of *β Geminorum*.
27	12	112½ + 2 to 112½ + 1	3	3	Path 1°
28	1880, Jan. 2	102 + 10½ to 102 + 9½	4	3	Path 1°
29	7	98 + 3	4	3	Path ½°
30	Feb. 8	148 + 22	5	4	
31	April 30	236 + 19 to 235 + 19	2	2	Path 1°
32	May 8	257 + 1	4	3	Slight movement.
33	11	181 − 8	4	3	Visible 1 sec.
34	June 29	316 − 11	4	4	Visible 1 sec. accurate.
35	July 30	10½ + 50 to 10½ + 51	2	4	Path 1°
36	Aug. 8	102 + 58	1	3	Slow; 1 sec.
37	9	21 + 39	4	4	
38	9	44¾ + 56¼	1	4	Visible 2 sec.
39	9	55 + 57	1	4	Visible 1 sec.
40	26	348 + 16½	3	4	Slight movement.
41	Sept. 29	6 + 5½ to 6 + 4½	5	3	Path 1°
42	30	359 − 14 to 358½ − 15	2	2	Path 1°
43	Oct. 8	27 + 36	4	4	
44	25	22 − 18	2	2	
45	31	54 + 7	2	4	Slow, 1 sec.

Catalogue of Radiant Points.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
1	1880, Jan. 5–10	140°+7°	7	3	Meteors generally bright with medium length of path and velocity, and leaving persistent streaks. 5 seen during half an hour on the 7th. Confirms Tupman's position at 145° + 5°, Jan. 4–11, '70.
2	2	227+48	6	2	Meteors quite bright, long, and generally rather slow, all recorded from 7 to 8 hours.
3	Feb. 6–8	130+22	3	3	Meteors faint, short, and rapid.
4	8, 9	155+20	3	3	Meteors rather bright, short, and rapid. Schiaparelli and Zezioli at 153°+21°. February 3.
5	1879, April 12–26	206−8	5	4	Meteors quite bright, short and rather slow, position very accurately determined from a 2nd mag. stationary meteor recorded on the 14th.
6	20–26	242+24	5	2	Meteors quite faint, short, and rapid. A feeble return of the Coronids.
7	20–22	245+50	3	3	Meteors quite faint, short, and rapid; position well deduced. Draconids No. I.
8	22–26	253+5	5	4	Meteors bright, short, and rapid: position well deduced from a stationary meteor = 1st mag. recorded on the 23rd.
9	19, 20	272+41½	4	4	Meteors quite bright, short, and rapid, with streaks: blue probably a branch of the Lyraids?
10	19–21	274+34	10	3	Meteors generally bright, short, and rapid, and of an orange colour. Lyraids, position quite well determined.
11	June 13–23	283−12	9	3	Meteors generally very brilliant, short, and of medium velocity, two = 0 in brightness and two = 1st mag.*: the brightest were of an orange colour.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
12	1879, June 13–24	307°+5°	4	3	Meteors bright, short, and rapid. Aquilids?
	1880, June 29	304+3	5	3	Meteors rather faint, generally rapid, and of medium length.
	29	295−1	4	3	Meteors generally quite bright, short, and rapid, possibly two distinct showers in this region?
13	1879, July 13–24	307−5	4		Meteors bright, short, and rapid. Aquilids. Compare with No. 12.
14	17–20	318+22	6	4	Meteors generally very bright, short, and rapid; position well determined.
15	13–24	338+18	5	4	Meteors generally faint, short, and rapid, position accurately deduced. Pegasids II.
16	1880, July 28	341−10	4	3	Meteors quite bright, short, and of medium velocity. Denning, = 341°−13° July 21–29.
17	28–31	330−6	20	4	Meteors very bright, and generally slow, position very accurately deduced. Max. 29–30. Aquariads.
18	28–30	328−15	8	4	Meteors generally rapid and very bright. Max. 28th, position accurately determined. Nos. 16, 17, and 18 possibly all one shower?
19	29–31	315+5	12	4	Meteors of medium brightness, short, and rapid; position accurately deduced. Max. 29. Active shower.
20	July 30, Aug. 1	294+13	8	3	Meteors of medium brightness, short, and rapid. Max. July 31. Confirms Heis at 294°+13° July 26–31.
21	30 1	303+8	4	2	Meteors very bright and slow; not accurate. Heis July 14, August 31 = 305°+7°.
22	29 7	356+25	6	2	Meteors of medium brightness, short, and rapid, not accurate.
23	29 1	331+53	5	3	Meteors generally bright, long, and rapid.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
		° °			
24	1880, Aug. 8, 9	5 + 75	6	3	Meteors very bright, short, and rapid.
25	1879, Aug. 10–12	5 + 57	5	3	Meteors very bright, short, and rather slow.
	1880, Aug. 8, 9	5 + 55	14	3	Meteors quite bright, of medium length and generally rapid with streaks. Active shower.
26	8, 9	$44\frac{3}{4} + 56\frac{1}{4}$	48	4	Meteors generally bright, short, and rapid with streaks; position very accurately determined from several short tracks near focus and one perfectly stationary meteor = 1st mag. and visible 2 seconds. Perseids. Horary No. on 8th 16↓s. 9th 28↓s. 8th = R.A. 38° + 56°.
	1879, Aug. 10–12	$44\frac{1}{2} + 57$	106	4	Meteors bright, short, and generally rapid with persistent streaks. Position very accurately determined from 20 paths exactly mapped. Horary No. 24 on 10th, 13 on 11th, and 5 on 12th. Very meagre display.
27	1880, Aug. 9	55 + 57	6	4	Meteors bright, short, and rapid with streaks. Position accurately deduced from several short tracks and one nearly stationary meteor. Probably branch of Perseids?
28	8, 9	270 + 69	4	4	Meteors bright, short, and slow, accurate. Heis, August 13–31 = 275° + 69°.
29	8, 9	332 + 60	7	3	Meteors generally not very bright, short, and rapid; principally seen on the 8th.
30	1879, Aug. 11	38 + 60	8	4	Meteors generally bright, short, and rapid; position very accurately deduced from 7 well-observed tracks, and one absolutely stationary of the 2nd mag. and visible 1 sec. Perseids.
31	10–12	17 + 42	4	2	Meteors bright, short, and rapid; strongly suspected.
32	10–12	23 + 37	4	3	Meteors rather bright, short, and rapid; position well determined.

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No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
33	1879, Aug. 10–12	180° + 78°	6	2	Meteors faint, long, and rapid; strongly suspected.
34	7–12	344 + 42	6	4	Meteors rather light, short and rapid; position accurately deduced. Honorids.
35	7–12	358 + 55	7	3	Meteors very bright, short, and rapid; position quite well determined, possibly same as No. 25?
36	20–23	240 + 12	4	3	Meteors rather bright and short with a medium velocity.
37	20–23	258 + 20	7	3	Meteors faint, short, and rapid.
38	1880, Aug. 28, Sept. 4	352 + 27	7	3	Meteors quite bright, short, and slow.
39	28 2	348 + 17	3	4	Meteors bright, short, and rapid; accurate.
40	31 2	2 + 16	5	3	Meteors short, not very bright, and rapid.
	1879, Sept. 9–11	2 + 15	9	4	Meteors generally faint, short, and rapid: accurate, principally seen on the 11th. Pegasids III. Heis = 1° + 14° September 16–31.
41	15–20	10 + 35	10	4	Meteors faint and rapid, with medium length of path; 5 of the number seen on the 17th. Andromedes I.
42	7–17	346 + 45	8	3	Meteors generally faint and rapid, with medium length of path. Radiant point appeared to shift from 343° + 45° to 349° + 45°.
43	1880, Sept. 25, Oct. 5	8 + 22	7	2	Meteors short, quite bright, and very slow; rather diffuse.
44	29 9	24 + 17	10	4	Meteors generally bright, short, and quite slow; average length of path 7° and duration about 1 sec. Very close agreement with the radiant point of the comet of 1769 = 24½° + 17° September 28.
45	Oct. 1	16 + 50	4	2	Meteors short, faint, and rapid; strongly suspected.
46	2–5	15 + 40	5	3	Meteors not very bright, rather short, and slow.

No.	Date.	Position. R.A. Dec.	Nos. of ↓'s.	Weight.	Remarks.
		° °			
47	1880, Oct. 5–8	27 + 24	5	3	Meteors bright, generally short, and rapid.
48	6, 7	50 + 10	4	3	Meteors quite bright, of medium length, and slow.
49	1879, Oct. 14–19	23 + 20	4	4	Meteors rather bright, short, with medium velocity; position well determined; one nearly stationary meteor.
50	19	26 + 48	4	3	Meteors faint, short, and rapid.
51	19	44 + 18	4	3	Meteors rather bright, short, with medium velocity. Denning October 2–19, '77 = 47° + 16°.
52	14–19	44 + 35	5	4	Meteors rather bright, short, and rapid; position well deduced.
53	19	48 − 4	4	3	Meteors bright, short, and rather slow; one nearly stationary.
54	19, 20	59 + 14	5	3	Meteors rather bright, short, and rapid. Taurids I.
	1880, Oct. 31, Nov. 3	60 + 18	5	3	Meteors quite bright, of medium length, and rapid.
	1879, Nov. 7–10	60 + 19	6	3	Meteors very bright, and generally slow, with long paths, orange coloured.
55	Oct. 19, 20	59 − 13	4	3	Meteors very bright, slow, and short. Tupman, October 5–6, '69 = 54° − 14°.
56	14–20	73 − 53	8	2	Meteors generally bright, slow, and of medium length, not accurate.
57	19	84 − 26	4	4	Meteors rather bright, short, and rapid; well determined.
58	19	92 + 15	8	4	Meteors not very bright, short, and rapid, with streaks; position very accurately deduced. Orionids.
59	19	350 + 50	4	3	Meteors faint, short, and rapid.
60	1880, Oct. 24	26 + 13	6	4	Meteors of medium brightness and length, and quite slow; accurate.
61	25 − 31	3 + 42	7	3	Meteors short, faint, and rapid.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
		° °			
62	1880, Oct. 31, Nov. 2	54 + 7	6	3	Meteors very bright, short, and slow.
63	24 3	43 + 24	7	3	Meteors short, slow, and bright.
	1879, Nov. 7–10	47 + 24	7	4	Meteors quite bright and short, with medium velocity, position very accurately determined from several short tracks, and one nearly stationary.
64	1880, Oct. 29, Nov. 3	325 + 65	4	2	Meteors of medium length, bright, and slow, with streaks, orange; strongly suspected.
65	1879, Nov. 10	42 + 43	4	3	Meteors faint and rapid, with medium length of path: strongly suspected.
66	10	65 + 30	5	3	Meteors bright, short, and rather slow; orange.
67	15	151 + 22	7	4	Meteors of medium brightness, short, and rapid, with streaks accurate. Leonids.
	1880, Nov. 11, 12	148 + 22	12	3	Meteors quite bright, short, and rapid, with streaks; Leonids.
68	1879, Nov. 15	146 + 1	3	3	Meteors short, and rather bright, orange; strongly suspected.
69	1880, Nov. 21–28	81 + 23	5	3	Meteors rather bright, of medium length, and slow. Taurids II.
	1879, Dec. 7–12	82 + 23	5	3	Meteors rather bright, of medium length and velocity. Taurids II.
70	7–12	75 + 45	5	3	Meteors short and bright; and radiant point very near α *Aurigæ*; strongly suspected.
71	7–12	100 + 35	13	3	Meteors generally faint, long, and rapid; position well deduced; possibly a branch of the Geminids?
72	7–12	110 + 33	20	4	Meteors bright, very short, and rapid; position well determined. Geminids.

The Binary Star, β Delphini (=β 151). By S. W. Burnham, Esq.

As a wide pair, *β Delphini* has been known as a double star since the time of Sir William Herschel, who detected the well-known companion, then about 30″ distant. Sir John Herschel subsequently found a fainter star a little nearer, the distance of which is now 28″ (the other being 35″). The measures show that both are mere optical companions, and have no physical relation to the principal star, the change in their positions being due to the proper motion of the bright star. In 1873, with an aperture of only 6 inches, I discovered that the principal star was composed really of two stars, differing in brightness something more than one magnitude. The distance then made it too easy a pair to escape attention with a small aperture. It very soon appeared from observations that the angle was increasing, while the stars were gradually approaching each other. It became more and more difficult, until finally, in 1879, it was single, or practically so, with the 18½-inch refractor of the Dearborn Observatory. This year it has commenced to open again, and although of the last degree of difficulty, and only observable under the best atmospheric conditions, I have measured it on four nights, with results as accordant as could be expected under the circumstances. The separate observations are as follows:—

P = 139°·5	D = 0″·28	1880·592
129·4	0·22	·613
127·1	0·30	·627
129·0	0·24	·766
142·9	0·27	·782

These distances are decidedly too large, as would be expected in attempting to measure a distance so small in comparison with the thickness of the micrometer-wires. The distance was estimated as not exceeding 0″·2. On each occasion the close pair of δ *Equulei* was measured, and that pair, the distance of which is 0″·35, was certainly at least double the distance of *β Delphini*.

The following measures will show the relative position of the companion, each year since its discovery, from the measures of Baron Dembowski and myself:—

β	1873·60	P = 355° ±	D = 0″·7 ±	Est.
Δ	74·66	15·6	0·61	5 nights.
Δ	75·65	20·1	0·54	4 „
Δ	76·66	25·8	0·48	4 „
β	77·79	40·8	0·32	4 „
β	78·65	53·7	0·24	4 „
β	79·54	Elongation, if any, in 90°; but very doubtful. Very good night.		
β	79·57	No certain elongation. First-class night.		
β	80·68	133·6	0·26	5 nights.

The observations show conclusively that we have here one of the most rapid binary systems known, the angular motion in the last two years being not less than 80°. It is too early to speculate upon the probable period, but the measures of the next three or four years will go far to give at least an approximate determination. It is to be hoped that double-star observers will give this interesting system special attention. By next year it will probably be comparatively easy of measurement. An increase in the distance of one-tenth of a second will be sufficient for this.

Chicago,
1880, *December* 15.

Remarks on Dr. Hartwig's Recent Determination of the Lunar Physical Libration. By the Rev. C. Pritchard, Savilian Professor of Astronomy at Oxford.

It will be in the recollection of the Society, that in June last I announced the completion of the mathematical and computational parts of the investigation of the Lunar Physical Libration, as derived from 59 measures of Lunar Photographs, and that another similar set, made with reference to a second spot upon the Moon, was nearly finished. Meanwhile Dr. Hartwig has just published a memoir detailing the results of 42 measures, made at Strassburg with a Heliometer, for the attainment of the same object. That memoir, I do not doubt, deserves and will find an honourable place in the Annals of Astronomy, and he will himself meet with the sympathy of astronomers on the successful termination of his labours in this behalf.

I find that the results obtained by Dr. Hartwig are in substantial, though not absolute, agreement with my own. An absolute agreement was scarcely to be expected, owing, not to the difficulty and to the length of the various processes employed, but to the fact that the principal quantity ultimately sought for could not considerably exceed a second of arc when seen from the Earth; and, independently of this fact, any inevitable errors in certain parts of the processes must entail 250 times the same amount of error in the ultimate result.

I will now so far anticipate the communication which I hope speedily to lay before the Society, as to place in juxtaposition some of the principal results of the researches in which we, and a few other astronomers, have been engaged.

The first of these researches was that published by Bouvard, at Paris, in 1822; the second by Kreil, at Milan, in 1837; the third by Wichmann with the Königsberg Heliometer in 1848; the fourth by Hartwig, with a Heliometer at Strassburg in 1880; and, finally, that by myself at Oxford, with photographs taken

in the focus of the De la Rue Reflecting Telescope, of which notice was given in the summer of 1880.

First, as to the mean Inclination (I) of the Lunar Equator to the Ecliptic; secondly, as to the maximum amount of the Lunar Physical Libration in Selenographic Longitude (L).

	I.	L.
	° ′ ″	″
Paris Determination	1 28 45	289·7
Milan	1 35 48	342·6
Königsberg	1 32 9	389·
Strassburg	1 36 34	207·
Oxford	1 32 58	350·

With regard to the last three, I do not think that, taking all the elements of the case into consideration, there exists ground for expecting a closer agreement, the reasons for which opinion will be stated in the sequel.

It is now necessary to state that the entire foundation of the researches hitherto undertaken, has been the measurement of the distances of some well-defined spot on the Moon, from various points in its periphery; and, in general, seven or eight such measures have been taken for a single observation. Wichmann and Dr. Hartwig selected the same spot, Mösting A. At Oxford, two spots have been selected, one of which, Triesnecker B, has the advantage of being much nearer to the Moon's mean centre than is Mösting A. In the course of the investigation the Selenographical Longitudes and Latitudes of these spots become necessarily determined. The results obtained by Wichmann and Dr. Hartwig are not in absolute accordance. They are as follows:—

	Wichmann.	Hartwig.
	° ′ ″	° ′ ′
Selenographic Longitude of Mösting A . . .	5 13 23	5 10 57
Selenographic Latitude	3 10 55	3 10 21

These coordinates imply a discordance of nearly three minutes of a great circle, as seen from the Moon's centre, but as seen from the Earth they indicate an uncertainty of a little under a second of arc. It may here be proper to observe that notwithstanding these different determinations of the coordinates of the same spot by two highly accomplished and successful observers, each employing upwards of forty distinct determinations, Dr. Hartwig comes to the conclusion that the probable error of a single determination of place, as seen from the Earth, is only $\pm 0''.43$. I think it becomes a question whether the discordance may possibly and properly be assigned to the use of instruments having different apertures. The fact exists. Does this also tend to explain the discordances in the determination of the inclination of the Moon's Equator to the Ecliptic?

It is also desirable to remark on Dr. Hartwig's method of avoiding errors arising from any irregularities in the contour of the Moon's limb. He takes seven places on the limb, distributed with considerable uniformity; but, in making a measure of the distance of the spot with the Heliometer, wherever his eye could perceive the necessity for it, he has regard to what appears in his judgment to be the average level of the contour.* I have myself adopted a different method, which is this: inasmuch as the photograph lies quietly and undisturbed under the microscope of the De la Rue Micrometer, an opportunity is given of leisurely selecting those places where little or no serrature exists. I may add also that, in the case of photography, there are no changes of the Moon's diameter during the time of observation. Changes of this nature necessarily introduce considerable trouble in the heliometer determinations.

While on this subject of the irregularities of the Moon's limb it is proper to refer to what Dr. Hartwig states as the result of his own experience. He altogether and decidedly declines to accept what he terms the emphatically expressed conviction of Mr. Neison as regards the introduction of serious periodical errors from this source. My own long experience is in entire accordance with the opinions of Dr. Hartwig.†

I may also add that after listening to Mr. Neison's strictures on this behalf, I instituted a series of measures round the entire visible periphery of a Lunar photograph, avoiding no irregularities whatever; and the result was such as convinced me that the irregularities themselves, in this particular case, did not interfere with the exactness of computed results. But there was another conclusion which was unavoidably impressed on my own mind, from these and similar researches. Dr. Hartwig, in the course of his admirable Memoir, refers to the uncertainty of the determination of the Selenocentric Longitudes when compared with those of the Selenocentric Latitudes. This comparative insecurity which has been pointed out by Wichmann, had been sensibly observed by myself. Experiments at last indicated its probable cause; and, towards the conclusion of the work, I discovered that the source of error might be very much avoided by increasing the number of measures, and by distributing them in particular and ascertainable directions. The true cause originates, I believe, in the necessary want of symmetry of the Moon's illuminated periphery, with respect to the selected spot on the Moon's disk. The configuration of the spot, relative to the illuminated periphery, is always unavoidably one-sided in Longitude. Owing to this cause, Dr. Hartwig, and Wichmann before him, considered themselves justified in applying certain weights (more or less arbitrarily determined) to the Latitude equations of condition. I have myself hesitated, and am still

* Cf. p. 27, Dr. Hartwig's *Memoir*.
† Page 31, *Op. cit.*

inclined to hesitate, to adopt this process; but I fear I must now reluctantly acquiesce in this method, in order to place the results in fair juxtaposition. Should I undertake, as I propose, to complete another set of 100 measures, I intend to take ten individual measures, properly selected, for each determination; and then I shall decline to weight the Selenocentric Latitude equations.

The general conclusions which in my judgment are arrived at by a comparison of the Strassburg with the Oxford researches are these :—

1. There does exist a real, but inconsiderable, libration in Longitude, less than five minutes of arc, and probably not ascertainable within some decades of seconds.

2. The Moon is sensibly spherical.

3. There appears but little hope of ascertaining the position of a spot upon the Moon with sufficient accuracy to admit of its use, in preference to the limb, in astronomical observations of the Moon's place.

In conclusion, I beg to tender Dr. Hartwig my hearty congratulations on the successful termination of a research, of the difficulties and delicacy of which I have a painful familiarity.

Oxford,
1881, *Jan.* 13.

Description of an Observing Seat for an Equatoreal. By Professor G. W. Hough, Director of the Dearborn Observatory.

In making micrometer measures with the equatoreal telescope, it is desirable to have a convenient seat for the observer. A person in a cramped or unnatural position can hardly do as good work as when the body is resting in an easy and natural manner.

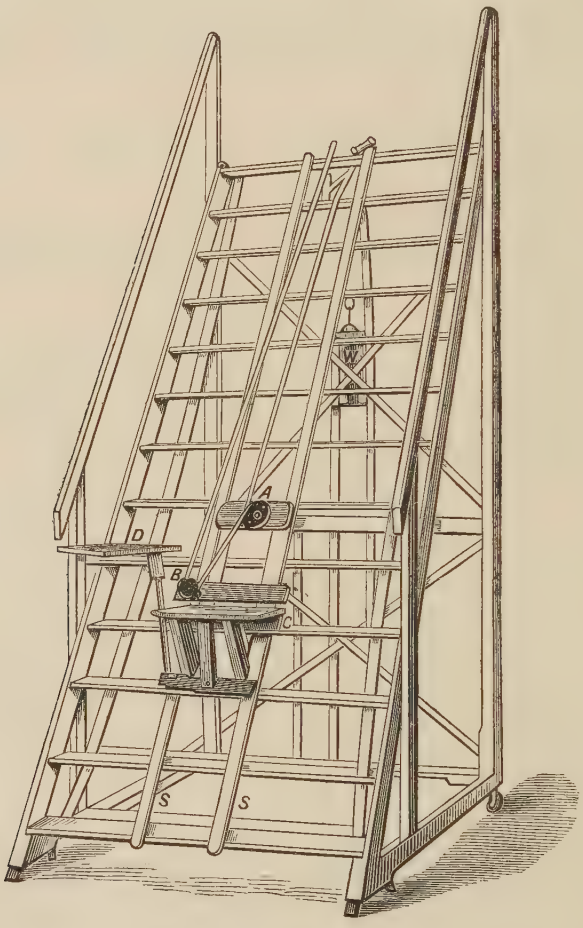
The problem of a perfect seat for the Equatoreal can hardly be said to have been solved, although a number of elaborate and costly chairs have been constructed by different observers. I presume the majority of those observing with a large Equatoreal use a simple step-ladder, provided with various cushions, &c., for securing the proper elevation of the eye. This simple apparatus seems to be the most expeditious for actual work, notwithstanding the trouble of always getting in just the right position.

The Dearborn Equatoreal was provided with an elaborate and costly chair, constructed after the model of that devised by Bond, for the Harvard University telescope.

This piece of mechanism was found to be so unwieldy, and

required so much expenditure of muscle to bring the seat in position, and moreover consumed so much time for its manipulation, that practically it has never been of any use. Nearly all observations have, accordingly, been made by the use of an ordinary step-ladder.

During the past year Mr. S. W. Burnham and myself



decided that an adjustable seat attached to the step-ladder would add greatly to the convenience of the observer, and would also economise the time in preparing for micrometer measures. A light and easily manipulated chair is especially desirable for double-star work, since many different objects are often examined on the same night.

After considering the problem, the following mechanism was devised, which, after some months of constant use, seems to answer the purpose admirably.

The accompanying drawing will clearly show the seat mechanism.

Over the central portion of a step-ladder 11 feet in height and 5 feet in width, are screwed two strips of pine, S, S, 2 inches in width, which pass from the top to the bottom of the steps; the distance between them being about 7 inches.

A light wooden frame is made to slide on this track, to which the observing seat, C, is attached. The seat is 20 inches in length and 12 inches in depth. In order to keep it in place, the tracks S S are bevelled on the inside, and the frame is made with a corresponding bevel like an ordinary slide-rest.

The seat, when in use, is raised and lowered by means of the following device.

To the top of the steps is attached a woven rope, three-eighths of an inch in diameter, which is passed *once* around a fixed drum, A, 5 inches in diameter, attached to the seat-frame, thence over a loose pulley, B, thence back again to the top of the steps over a pulley, where a weight, W, is attached just sufficient to balance the seat. The weight W slides in vertical tracks attached to the rear of the step-ladder.

The seat, therefore, is held in position simply by the *friction* of the cord around the fixed drum A, without the use of any clamp or stop.

The whole weight of the seat and frame is about 15 lbs. A very slight force, viz. 10 lbs., will cause it to slide up the track when unloaded.

When seated on the chair, a slight pull downwards, on the rope to which the counterpoise is attached, causes it to descend any amount desired. As the whole merit of the apparatus consists in the ease and celerity with which the observer may secure the proper elevation for the eye, it may be added that the seat is completely under control; it may be moved an inch, or a foot, almost instantaneously, and in this respect is infinitely superior to any screw or rack motion.

For the upward movement it is necessary to rest one's weight on the steps.

As there are no ratchets or stops, but simply a rope wrapped round a smooth drum, the chair can be manipulated in the dark without difficulty. The cost of applying this apparatus to the step-ladder was about five dollars.

When using the micrometer, a small table, D, for holding the note-book, lamp, eye-piece, &c., is attached on the right-hand side of the seat, and may be removed at pleasure. The step-ladder itself is mounted on castors and is easily shifted to any required position.

When micrometer measurements are not in progress, the seat may be slid to the top of the steps, where it is out of the way, and the ladder used in the ordinary way.

A step-ladder built to conform to the radius of the telescope and provided with a sliding seat similar to the one described, could be mounted on tracks so as always to be at the right distance from the telescope, but we doubt whether it would be as convenient as one free to move over the floor in any direction.

Chicago, U.S.A.
1880, *December*.

Note on the comparative Brightness of the Nebula of Orion.

By E. B. Knobel, Esq.

At the January meeting of this Society a photograph of the Nebula of *Orion*, by Dr. H. Draper, was exhibited, and remarks were made upon it which appear to require a slight correction. It was stated by Mr. Ranyard that, "comparing the photograph with the drawings of Bond, Secchi, Liapounov, and others, it corresponds with none of them. The most curious difference is

the little bright nucleus preceding the trapezium, which is the brightest part in the photograph, but that is not the case in *any* of the drawings." And further the remark was made that "Dr. Draper's photograph shows that a nebulous mass on the preceding side of the trapezium is the brightest region of the nebula. This does not correspond with any of the drawings."

In the engraving of Bond's drawing which I possess, the brightest portion of the nebula appears certainly to be the mass on the preceding side of the trapezium; and in one of the most recent and carefully executed drawings of this nebula, by M. Trouvelot, made with the 26-inch Equatoreal, and published in the "Description of the Instruments of the United States Naval Observatory, Washington, 1876," the portion of the nebula preceding the trapezium is shown very considerably brighter than

any other part of it, confirming Bond's drawing and Dr. Draper's photograph.

I have endeavoured to make a few photometric observations for the purpose of determining the brightest portion of this nebula. With that object I have employed an $8\frac{1}{2}$-inch silvered glass reflector with an *unsilvered* glass flat. The light is thus very considerably reduced; and then by applying the limiting-aperture astrometer, described in *Monthly Notices*, vol. xxxv. p. 100, I am able to extinguish successively every portion of the nebula, and by this means obtain an approximation to the light-ratio of its several masses.

Bad weather has prevented my securing very many results, as it is so necessary that such observations should not be undertaken except on very fine, clear nights. The power used was 120.

1881, Feb. 28. Fine clear night. Seeing good from 7 to 9 P.M.

Aperture $8\frac{1}{2}$ inches. The portion of the nebula of *Orion* surrounding the ray marked γ in the diagram is faintly visible.

Aperture 4·92 inches. The ray γ extinct. The limiting aperture for γ was not determined, but the ray could not now be glimpsed.

The only nebula visible consists of the central portions marked α and β. The portion β extends north preceding the trapezium rather more than the distance from Star 1 to the trapezium, and at an average position angle of 340° or 350°, as roughly indicated by the dotted line in the woodcut.

Aperture 3·37 inches. Nebula faintly visible to nearly the same extent as in the preceding observation. Portion α much fainter than β.

Aperture 3·01 inches. Star 3 faintly visible.

Aperture 2·52 inches. Star 3 extinct. Portion α of nebula seen by averted vision. The only nebula visible directly, stretches from the trapezium in a north preceding direction.

Aperture 2·10 inches. Portion α of nebula extinct. A slight haze visible north preceding the trapezium.

Aperture 1·67 inches. All nebula extinct by direct vision, a very slight haze glimpsed by averted vision at the spot marked by the letter β.

Aperture 1·33 inches. Not a trace of any nebula or haze.

March 8, 7 P.M. to 8.30. Fine and clear, no haze. Moon 8 days old.

Apertures $8\frac{1}{2}$, 4·92, and 3·37 inches confirm the observations of Feb. 28.

Aperture 2·65 inches. Portion α of nebula is glimpsed by averted vision. The only nebula visible looking direct is the part marked β.

Aperture 1·82 inches. Portion α of nebula extinct. β faint.

Aperture 1·46 inches. Star 3 extinct; only two stars seen in trapezium. Slight haze on north preceding side of trapezium.

Aperture 0·91 inches. Nebula quite extinct; only one star distinctly seen in trapezium. Star 2 seen with difficulty.

For comparison, the objects not differing much in altitude, I made the following observations of the *Andromeda* nebula, power used being 80:—

March 8.

Aperture $8\frac{1}{2}$ inches. The nucleus of the nebula is round and bright, like Trouvelot's drawing in the Harvard College Series.

Aperture 2·46 inches. The only nebula visible is a small round luminous spot, the diameter of which is about one-half that seen with $8\frac{1}{2}$ inches aperture.

Aperture 1·95 inches. Nebula absolutely extinct. No glimpse of it by any means.

The mass α disappears uniformly, without apparently diminishing in extent; but the dotted portion, marked β, becomes smaller as the light is reduced, the brightest part being at the position marked in the diagram by the letter β.

In the above observations, it should be noted that the actual apertures which would extinguish the portions of the nebulæ referred to, are probably greater than those I have given, my object being to proceed by wide steps so as to be able to say with certainty that with one aperture the object was visible, and with the second aperture the object was not visible. It would have required many more observations than it was possible for me to make, to determine the exact aperture when each feature became extinct.

If we assume for the purpose of a rough approximation, that 1·8 in. and 1·3 in. are the limiting-apertures respectively of portions α and β of the nebula of *Orion*, then these will give a light ratio of 0·5216. And comparing the nebula of *Orion* with that in *Andromeda* in the same way, we have rough limiting-apertures of 1·33 in. for the former, and 1·95 in. for the latter, which give a light-ratio of 0·4652.

Though these figures must be received with caution, we may, I think, place more confidence in the proof that the central portion of the nebula of *Orion* preceding the trapezium is considerably brighter than that on the following side; and that the nebula in *Andromeda*, though so conspicuous to the naked eye, is decidedly less bright than the central portion of that in *Orion*.

I venture to think that this class of observation affords a far

more reliable method for determining any change in a nebula than the comparison of drawings or photographs, at any rate until the art of celestial photography is very much improved.

1881, *March* 11.

Further Notes on the Nebula in the Pleiades. By Maxwell Hall, Esq.

My first opportunity for observing the Nebula in the *Pleiades* since my last visit to England occurred on the night of the 21st November this year; meteorological work had detained me in Kingston.

As soon as the telescope was turned to *Merope*, a change in the preceding boundary of the Nebula was noticed; it was found to extend as far as *Electra*, and the parabolic form of the Nebula, as seen in 1877, was destroyed. (*Monthly Notices*, vol. xl. p. 89.)*

The new form of the Nebula was carefully drawn that night, and a copy of this drawing is sent herewith, together with the former drawing of 1877 for the sake of comparison.† Observations made on other nights confirmed the new boundary.

On the night of Dec. 2 the *Pleiades* were carefully examined with reference to the drawing made by Mr. Common at Ealing (*Monthly Notices*, vol. xl. p. 376); but I failed to make out any of its features.

The Supplementary Number of the *Monthly Notices* reached me a few days ago, and I was greatly pleased with Prof. Tempel's beautiful drawing of the *Pleiades* and the *Merope* Nebula (vol. xl. p. 622); his remarks are also very valuable.

Now, it will be seen that the form of the Nebula, as drawn by Prof. Tempel, is exactly the mean of the two forms drawn by me in 1877 and in 1880; and although Prof. Tempel says that he has never observed any change in the Nebula, yet I cannot refrain from asking him in these pages whether there is not, at the present time, an extension of the Nebula in the direction of *Electra*. As I remarked before, the Nebula has been seen to extend beyond *Electra* as far as *Celæno*; and I am anxious that the present indications of systematic change should be confirmed by Prof. Tempel and other observers.

Dr. Wolf has also favoured us (*Monthly Notices*, vol. xl. p. 293) by calling attention to his description and chart of the *Pleiades* published in the *Annales de l'Observatoire de Paris*; but unfortunately the volume in question was not in the library of the Royal Astronomical Society during my visit to England, and I have to gather his views from the few remarks made in the

* The drawing was published in the next No. of the *Monthly Notices*, p. 169.

† The new drawing shows an extension from the Nebula from above *Merope* (23) to *Electra* (17), and then nearly N. to S., uniting itself with the parabolic figure.—Ed.

Monthly Notices. The appearance of nebulosity throughout the whole group, excepting small circular areas concentric with the brighter stars, is indeed very striking; but on this subject we shall do well to await the further communications of Prof. Tempel.

Jamaica,
1880, *December* 13.

The Nebulæ in the Pleiades. By Henry Pratt.

After comparing the great differences of representation of these objects as seen by Maxwell Hall's 4-inch refractor and Mr. Common's 37-inch reflector, I was about to send a copy of my drawing made in October 1876, as giving the more usual appearance of the large Nebula (Tempel's) in a telescope of moderate aperture, when happily Signor Tempel's beautiful map of the whole group of the *Pleiades* appeared. Therein I find the most perfect representation of the Nebula as shown by my 8·15-inch With-Browning speculum, with one slight exception. I always see the Nebula extend to some little distance beyond *Electra* in a fainter condition than the portion drawn by Tempel, the latter corresponding to the brighter part of my drawing. As several observers appear to be still unsuccessful in their search for this object, I wish to say that during the last few years I have never looked at the *Pleiades* without also seeing the Nebula. Possibly a suitable power has something to do in the matter. I see it well with 45, Kellner, giving a field of 85′. but I prefer the view rendered by 84, Kellner, with a field of 50′. The light of the brighter stars near is quite sufficient to obliterate every trace of the small detached Nebula (Common's) between *Alcyone* and *Merope*. Indeed, as Signor Tempel with a larger telescope than mine, and an Italian sky, appears to question its existence, it may be doubted whether it is not impossible for so small a telescope as mine to show it at all. But I wish to say a word in favour of our foggy land, for in practice I find a very simple contrivance sufficient to render the existence of this small Nebula apparent even in my telescope. By applying a slotted diaphragm to power 84, so arranged as to exclude *Merope* and *Alcyone* and to show nearly the whole space between them, I am able to see just a trace of nebulosity where Mr. Common has drawn it. It is so faint, and fades so gently, that I cannot well define its boundary, but the major axis of its brightest part lies in about the direction given on Mr. Common's drawing, and I am able to see decidedly the deeper blackness of the sky between this Nebula and *Merope*. The faint stars mentioned by Mr. Common and Signor Tempel, and suggested by the latter to be the illusory cause of the nebulous appearance, I also distinctly see.

18 *Preston Street, Brighton,*
1881, *January* 11.